PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

L O N D O N.

VOL. LXXX. For the Year 1790.

PART I.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCXC.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the Chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHI-

THE PRESIDENT and COUNCIL of the Royal Society adjudged, for the Year 1789, the Medal on Sir GODFREY COPLEY's Donation to Mr. WILLIAM MORGAN, for his Two Papers on the Values of Reverſions and Survivorſhips, printed in the two laſt Volumes of the Philoſophical Tranſactions.

ERRATA.

P. 43. The title of this Paper ſhould ſtand thus: "Extract of a Letter from the Rev. *F. J. H. Wollaſton*, M. A. F. R. S. (dated Sydney College, Cambridge, Feb. 24, 1784.) to the Rev. *Francis Wollaſton*, LL.B. F. R. S. containing the Obſervation of a luminous Arch."

P. 93. l. laſt. Inſert "I" before "ſaw"

PHILOSOPHICAL TRANSACTIONS.

I. *Account of the Diſcovery of a Sixth and Seventh Satellite of the Planet Saturn; with Remarks on the Conſtruction of its Ring, its Atmoſphere, its Rotation on an Axis, and its ſpheroidical Figure.* By William Herſchel, *LL.D. F. R. S.*

Read November 12, 1789.

IN a ſhort Poſtſcript, added to my laſt Paper on Nebulæ, I announced the diſcovery of a *ſixth ſatellite* of Saturn, and mentioned, that I intended to communicate the particulars of its orbit and ſituation to the Members of the Royal Society, at their next meeting. I have now the honour to preſent them, at the ſame time, with an account of two ſatellites inſtead of one; and have called them the *ſixth* and *ſeventh*, though their

ſituation in the Saturnian ſyſtem intitles them, very probably, to the firſt and ſecond place. This I have done to the end that in future we may not be liable to miſtake, in referring to former obſervations or tables, where the five known ſatellites have been named according to the order they have hitherto been ſuppoſed to hold in the range of diſtance from the planet.

It may appear remarkable, that theſe ſatellites ſhould have remained ſo long unknown to us, when, for a century and an half paſt, the planet to which they belong has been the object of almoſt every aſtronomer's curioſity, on account of the ſingular phænomena of its ring. But it will be ſeen preſently, from the ſituation and ſize of the ſatellites, that we could hardly expect to diſcover them till a teleſcope of the dimenſions and aperture of my forty-feet reflector ſhould be conſtructed; and I need not obſerve how much we Members of this Society muſt feel ourſelves obliged to our Royal Patron, for his encouragement of the ſciences, when we perceive that the diſcovery of theſe ſatellites is intirely owing to the liberal ſupport whereby our moſt benevolent King has enabled his humble aſtronomer to complete the arduous undertaking of conſtructing this inſtrument.

The planet Saturn is, perhaps, one of the moſt engaging objects that aſtronomy offers to our view. As ſuch it drew my attention ſo early as the year 1774; when, on the 17th of March, with a 5½-feet reflector, I ſaw its ring reduced to a very minute line, as repreſented in fig. 1. (Tab. I.) On the 3d of April, in the ſame year, I found the planet as it were ſtripped of its noble ornament, and dreſſed in the plain ſimplicity of Mars. See fig. 2. I paſs over the following year, in which, with a 7-feet reflector, I ſaw the ring gradually open, till it came to the appearance expreſſed in fig. 3. (Tab. II.) the

original of which was delineated from nature, on the 20th of June, 1788, by means of a very good 10-feet reflector.

It ſhould be noticed, that the black diſk, or belt, upon the ring of Saturn is not in the middle of its breadth; nor is the ring ſubdivided by many ſuch lines, as has been repreſented in divers treatiſes of aſtronomy; but that there is one ſingle, dark, conſiderably broad line, belt, or zone, upon the ring, which I have always permanently found in the place where my figure repreſents it. I give this, however, only as a view of the northern plane of the ring, as the ſituation of the planet has hitherto not afforded me any other. The ſouthern one, which is lately come to be expoſed to the ſun, will ſhortly be opened ſufficiently to enable me to give alſo the ſituation of its belts, if it ſhould have any.

From my obſervations it appears, that the zone on the northern plane of the ring is not, like the belts of Jupiter or thoſe of Saturn, ſubject to variations of colour and figure; but is moſt probably owing to ſome permanent conſtruction of the ſurface of the ring itſelf. That however, for inſtance, this black belt cannot be the ſhadow of a chain of mountains, may be gathered from its being viſible all round on the ring; for at the ends of the anſæ there could be no ſhades viſible, on account of the direction of the ſun's illumination, which would be in the line of the chain; and the ſame argument will hold good againſt ſuppoſed caverns or concavities. It is moreover pretty evident, that this dark zone is contained between two concentric circles, as all the phænomena anſwer to the projection of ſuch a zone. Thus, in fig. 4. which was taken the 11th of May, 1780, we may ſee, that the zone is continued all round the ring, with a gradual decreaſe of breadth towards the middle,

anſwering to the appearance of a narrow circular plane, projected into an ellipſis.

As to the ſurmiſe, which might occur to us, of a diviſion of the ring, or rather of two rings, one about the other, with a diſtance of open ſpace between them, it does not appear eligible to venture on ſo artificial a conſtruction, by way of explaining a phænomenon that does not abſolutely demand it. If one ring, of a breadth ſo conſiderable as that of Saturn, is juſtly to be eſteemed the moſt wonderful arch that, by the laws of gravity, can be held together, how improbable muſt it appear to ſuppoſe it ſubdivided into narrow ſlips of rings, which by this ſeparation will be deprived of a ſufficient depth, and thus loſe the only dimenſion which can keep them from falling upon the planet? It is, however true, that as yet we do not know of the rotation of the ring, which may be of ſuch a proper velocity as greatly to aſſiſt its ſtrength; and that, in the ſubdiviſions, of courſe the different velocities for each diviſion may be equally ſuppoſed to keep them up. If the ſouthern plane ſhould prove to be very differently marked, it will at once remove every ſurmiſe of ſuch a diviſion; but if it ſhould offer us the ſame appearance of a dark zone, in the ſame ſituation, and of an equal breadth with the one I have obſerved on the northern ſide, I would ſtill remark, that, ſince a moſt effectual way to verify the duplicity of the ring is within our reach, it will be the beſt way to ſuſpend our judgement till that can be put to the trial. The method I allude to is an occultation of ſome conſiderable ſtar by Saturn, when, if the ring be divided, it will be ſeen between the openings, as well as between the ring and Saturn.

With regard to the nature of the ring, we may certainly affirm, that it is no leſs ſolid and ſubſtantial than the planet itſelf.

itſelf. The ſame reaſons which prove to us the ſolidity of the one will be full as valid when applied to the other. Thus we ſee, in fig. 3. and 4. the ſhadow of the body of Saturn upon the ring, which, in fig. 3. is eclipſed towards the north, on the following ſide, and in fig. 4. about the middle, according to the oppoſite ſituation of the ſun. In the ſame manner we ſee the ſhadow of the ring caſt on the planet, where in fig. 1. and 2. we find it on the equatorial part; and May 28, 1780, I ſaw it towards the ſouth. If we deduce the quantity of matter, contained in the body, from the power whereby the ſatellites are kept in their orbits, and the time of their revolution, it muſt be remembered, that the ring is included in the reſult. It is alſo in a very particular manner evident, that the ring exerts a conſiderable force upon theſe revolving bodies, ſince we find them ſtrongly affected with many irregularities in their motions, which we cannot properly aſcribe to any other cauſe than the quantity of matter contained in the ring; at leaſt we ought to allow it a proper ſhare in the effect, as we do not deny but that the conſiderable equatorial elevation of Saturn, which I ſhall eſtabliſh hereafter, muſt alſo join in it.

The light of the ring of Saturn is generally brighter than that of the planet: for inſtance, April 19, 1777, I ſaw the ſouthern part of the ring, which paſſed before the body, very plainly brighter than the diſk of Saturn, on which it was projected; and on the 27th of the ſame month, I found, that with a power of 410, my ſeven-feet reflector had hardly light enough for Saturn, when the ring was notwithſtanding ſufficiently bright. Again, the 11th of March, 1780, I tried the powers of 222, 332, and 449, ſucceſſively, and found the light of Saturn leſs intenſe than that of the ring; the colour of the body with the high powers turning to a kind of yellow,

while that of the ring ſtill remained white. The ſame reſult happened on June 25, 1781, with the power 460.

I come now to one of the moſt remarkable properties in the conſtruction of the ring, which is its extreme thinneſs. The ſituation of Saturn, for ſome months paſt, has been particularly favourable for an inveſtigation of this circumſtance; and my experiments have been ſo complete, that there can remain no doubt on this head.

When we were nearly in the plane of the ring, I have repeatedly ſeen the firſt, the ſecond, and the third ſatellites, nay even the ſixth and ſeventh, paſs before and behind the ring in ſuch a manner that they ſerved as excellent micrometers to eſtimate its thickneſs by. It may be proper to mention a few inſtances, eſpecially as they will ſerve to ſolve ſome phænomena that have been remarked by other aſtronomers, without having been accounted for in any manner that could be admitted, conſiſtently with other known facts. July 18, 1789, at 19 h. 41′ 9″, ſidereal time, the firſt ſatellite ſeemed to hang upon the following arm, declining a little towards the north, and I ſaw it gradually advance upon it towards the body of Saturn; but the ring was not ſo thick as the lucid point. July 23, at 19 h. 41′ 8″, the ſecond ſatellite was a very little preceding the ring; but the ring appeared to be leſs than half the thickneſs of the ſatellite. July 27, at 20 h. 15′ 12″, the ſecond ſatellite was about the middle, upon the following arm of the ring, and towards the ſouth; and the ſixth ſatellite on the farther end, towards the north; but the arm was thinner than either of them. Auguſt 29, at 22 h. 12′ 25″, the third ſatellite was upon the ring, near the end of the preceding arm; and my remark at the time when I ſaw it was, that the arm ſeemed not to be the fourth, at leaſt not the third, part of the diameter of the ſatellite, which, in the ſituation

ſituation it was, I took to be leſs than one ſingle ſecond in diameter. At the ſame time I alſo ſaw the ſeventh ſatellite, at a little diſtance following the third, in the ſhape of a bead upon a thread, projecting on both ſides of the ſame arm: hence we are ſure, that the arm alſo appeared thinner than the ſeventh ſatellite, which is conſiderably ſmaller than the ſixth, which again is a little leſs than the firſt ſatellite. Auguſt 31, at 20 h. 48′ 26″, the preceding arm was loaded about the middle by the third ſatellite. October 15, at 0 h. 43′ 44″, I ſaw the ſixth ſatellite, without obſtruction, about the middle of the preceding arm, though the ring was but barely viſible with my forty-feet reflector, even while the planet was in the meridian; however, we were then a little inclined to the plane of the ring, and the third ſatellite, when it came near its conjunction with the firſt, was ſo ſituated that it muſt have partly covered the firſt a few minutes after the time I loſt it behind my houſe. In all theſe obſervations the ring did not in the leaſt interfere with my view of the ſatellites. October 16, I followed the ſixth and ſeventh ſatellites up to the very diſk of the planet; and the ring, which was extremely faint, oppoſed no manner of obſtruction to my ſeeing them gradually approach the diſk, where the ſeventh vaniſhed at 21 h. 46′ 44″, and the ſixth at 22 h. 36′ 44″.

I might bring many other inſtances, if the above were not quite ſufficient for the purpoſe. There is, however, ſome conſiderable ſuſpicion, that, by a refraction through ſome very rare atmoſphere on the two planes of the ring, the ſatellites might be lifted up and depreſſed, ſo as to become viſible on both ſides of the ring, even though the ring ſhould be equal in thickneſs to the diameter of the ſmalleſt ſatellite, which may amount to a thouſand miles. As for the argument of its

incredible

incredible thinneſs, which ſome aſtronomers have brought from the ſhort time of its being inviſible, when the earth paſſes through its plane, we cannot ſet much value upon them; for they muſt have ſuppoſed the edge of the ring, as they have alſo repreſented it in their figures, to be ſquare; but there is the greateſt reaſon to ſuppoſe it either ſpherical or ſpheroidical, in which caſe evidently the ring cannot diſappear for any long time. Nay, I may venture to ſay, that the ring cannot poſſibly diſappear on account of its thinneſs; ſince, either from the edge or the ſides, even if it were ſquare on the corners, it muſt always expoſe to our ſight ſome part which is illuminated by the rays of the ſun: and that this is plainly the caſe, we may conclude from its being viſible in my teleſcopes during the time when others of leſs light had loſt it, and when evidently we were turned towards the unenlightened ſide, ſo that we muſt either ſee the rounding part of the enlightened edge, or elſe the reflection of the light of Saturn upon the ſide of the darkened ring, as we ſee the reflected light of the earth on the dark part of the new moon. I will, however, not decide which of the two may be the caſe; eſpecially as there are other very ſtrong reaſons to induce us to think, that the edge of the ring is of ſuch a nature as not to reflect much light.

I cannot leave this ſubject without mentioning both my own former ſurmiſes, and thoſe of ſeveral other aſtronomers, of a ſuppoſed roughneſs in the ſurface of the ring, or inequality in the planes and inclinations of its flat ſides. They aroſe from ſeeing luminous parts on its extent, which were ſuppoſed to be projecting points, like the moon's mountains; or from ſeeing one arm brighter or longer than another; or even from ſeeing one arm when the other was inviſible. I was, in the beginning of this ſeaſon, inclined to the ſame opinion, till one of theſe ſup-

poſed

poſed luminous points was kind enough to venture off the edge of the ring, and appeared in the ſhape of a ſatellite. Now, as I had collected every inequality of this ſort, it was eaſy enough for me afterwards to calculate all ſuch ſurmiſes by the known periodical time of the firſt, ſecond, third, ſixth, and ſeventh ſatellites; and I have always found that ſuch appearances were owing to ſome of theſe ſatellites which were either before or behind the ring. The 20th of October, for inſtance, at 22 h. 35′ 46″, I ſaw four of Saturn's ſatellites all in one row, and at almoſt an eqnal diſtance from each other, on the following ſide; and yet the firſt ſatellite, which was the fartheſt of them all, was only about half-way towards its greateſt elongation from the body of Saturn, as may be ſeen in fig. 5. (Tab. III.). How eaſily, with an inferior teleſcope, this might have been taken for one of the arms of Saturn, I leave thoſe to gueſs who know what a degree of accuracy it muſt require to diſtinguiſh objects that are ſo minute, and at the ſame time ſo faint, on account of their nearneſs to the diſk of the planet. Upon the whole, therefore, I cannot ſay, that I had any one inſtance that could induce me to believe the ring was not of an uniform thickneſs; that is, equally thick at equal diſtances from the center, and of an equal diameter throughout the whole of its conſtruction. The idea of protuberant points upon the ring of Saturn, indeed, is of itſelf ſufficient to render the opinion of their exiſtence inadmiſſible, when we conſider the enormous ſize ſuch points ought to be of, for us to ſee them at the diſtance we are from the planet.

From theſe ſuppoſed luminous points I am, by imperceptible ſteps, brought to the diſcovery of two ſatellites of Saturn, which had eſcaped unnoticed, on account of their little diſtance from the planet, and faintneſs; which latter is partly to be

aſcribed to their ſmallneſs, and partly to being ſo near the light of the ring and diſk of Saturn. Strong ſuſpicions of the exiſtence of a ſixth ſatellite I have long entertained; and, if I had been more at leiſure two years ago, when the diſcovery of the two Georgian ſatellites took me as it were off the ſcent, I ſhould certainly have been able to announce its exiſtence as early as the 19th of Auguſt, 1787, when, at 22 h. 18′ 56″, I ſaw, and marked it down as being probably, a ſixth ſatellite, which was then about 12 degrees paſt its greateſt preceding elongation. But, as I obſerved before, not having time to give my thoughts to the ſubject, I reſerved a full inveſtigation of the number of ſatellites, and the nature of the ring of Saturn for a future opportunity. Beſides, not having any tables of the ſatellites, I could not confidently ſay, whether the fifth ſatellite was not one of the five which I perceived in motion that night, though afterwards I found, that the real fifth had alſo been in view, and was marked down as a ſtar, by the letter *b*, in a figure I delineated of Saturn and its ſatellites that evening.

In the year 1788 very little could be done towards a diſcovery, as my twenty-feet ſpeculum was ſo much tarniſhed by *zenith ſweeps*, in which it had been more than uſually expoſed to falling dews, that I could hardly ſee the Georgian ſatellites. In hopes of great ſucceſs with my forty-feet ſpeculum, I deferred the attack upon Saturn till that ſhould be finiſhed; and having taken an early opportunity of directing it to Saturn, the very firſt moment I ſaw the planet, which was the 28th of laſt Auguſt, I was preſented with a view of ſix of its ſatellites, in ſuch a ſituation, and ſo bright, as rendered it impoſſible to miſtake them, or not to ſee them. The retrograde motion of Saturn amounted to nearly 4½ minutes *per* day,

which made it very eaſy to aſcertain whether the ſtars I took to be ſatellites really were ſo; and, in about two hours and an half, I had the pleaſure of finding, that the planet had viſibly carried them all away from their places. I continued my obſervations conſtantly, whenever the weather would permit; and the great light of the forty-feet ſpeculum was now of ſo much uſe, that I alſo, on the 17th of September, detected the ſeventh ſatellite, when it was at its greateſt preceding elongation.

As ſoon as I had obſervations enough to make tables of the motion of theſe new ſatellites, I calculated their place backwards, and ſoon found that many ſuſpicions of theſe ſatellites, in the ſhape of protuberant points on the arms, were confirmed, and ſerved to correct the tables, ſo as to render them more perfect. Fig. 6. repreſents the ſeven ſatellites of Saturn, as they were ſituated October 18, at 21 h. 22′ 45″. The ſmall ſtar *s* ſerved to ſhew the motion of the planet in a ſtriking manner; as, in about 3¾ hours after the above-mentioned time, the whole Saturnian ſyſtem was completely moved away, ſo as to leave the ſtar *s* as much following the ſecond and firſt ſatellites, which then were in conjunction, as it now was before the ſecond.

By comparing together many obſervations of the ſixth ſatellite, I find, that it completes a ſidereal revolution about Saturn in one day, 8 hours, 53′ 9″. And if we ſuppoſe, with M. DE LA LANDE *, that the fourth is at the mean diſtance of 3′ from the center of Saturn, and performs one revolution in 15 d. 22 h. 34′ 38″, we find the diſtance of the ſixth, by KEPLER's law, to be 35″,058. Its light is conſiderably ſtrong, but not equal to that of the firſt ſatellite; for, on the 20th of

* Aſtr. § 2996, 2997.

October, at 19 h. 56′ 46″, when theſe two ſatellites were placed as in fig. 7. the firſt, notwithſtanding it was nearer the planet than the ſixth, was ſtill viſibly brighter than the latter. It would, however, be worth while to try whether a good achromatic teleſcope, of a large aperture, might not poſſibly ſhew it at the time of its greateſt diſtance from the planet, and when no other ſatellite is near; that is, provided it will ſhew the other five ſatellites with great eaſe, as otherwiſe there will be no reaſon to expect it ſhould ſhew the ſixth.

In the period of this ſatellite I have employed the obſervation of the 19th of Auguſt, 1787, as, from other calculations, it ſeems the revolution is determined near enough to reach back ſo far.

The moſt diſtant obſervations of the ſeventh ſatellite, being compared together, ſhew, that it makes one ſidereal revolution in 22 hours, 40 minutes, and 46 ſeconds: and, by the ſame *data* which ſerved to aſcertain the dimenſion of the orbit of the ſixth, we have the diſtance of the ſeventh, from the center of Saturn, no more than 27″366. It is incomparably ſmaller than the ſixth; and, even in my forty-feet reflector, appears no bigger than a very ſmall lucid point. I ſee it, however, alſo very well in the twenty-feet reflector; to which the exquiſite figure of the ſpeculum not a little contributes. It muſt nevertheleſs be remembered, that a ſatellite once diſcovered is much eaſier to be ſeen than it was before we were acquainted with its place.

The revolution of this ſatellite is not nearly ſo well aſcertained as that of the former. The difficulty of having a number of obſervations is uncommonly great; for, on account of the ſmallneſs of its orbit, the ſatellite lies generally before and behind the planet and its ring, or at leaſt ſo near them that, except

except in very fine weather, it cannot eaſily be ſeen well enough to take its place with accuracy. On the other hand, the greateſt elongations allow ſo much latitude for miſtaking its true ſituation, that it will require a conſiderable time to divide the errors that muſt ariſe from imperfect eſtimations.

The orbits of theſe two ſatellites, as appears from many obſervations of them, are exactly in the plane of the ring, or at leaſt deviate ſo little from it, that the difference cannot be perceived. It is true, there is a poſſibility that the line of their nodes may be in, or near, the preſent greateſt elongation, in which caſe the orbits may have ſome ſmall inclination; but as I have repeatedly ſeen them run along the very minute arms of the ring, even then the deviation cannot amount to more than perhaps one or two degrees; if, on the contrary, the nodes ſhould be ſituated near the conjunction, this quantity would be ſo conſiderable that it could not have eſcaped my obſervation.

From the ring and ſatellites of Saturn we now turn our thoughts to the planet, its belts, and its figure.

April 9, 1775. I obſerved a northern belt on Saturn, which was a little inclined to the line of the ring.

May 1, 1776. There was another belt, inclined about 15 degrees to the ſame line, but it was more to the ſouth, and on the following ſide came up to the place in which the ring croſſes the body.

July 13, —— The belt was again depreſſed towards the north, almoſt touching the line where the ring paſſed behind the body.

April 8, 1777. There were two fine belts, both a little inclined to the ring.

June

June 20, 1778. There were two belts parallel to the ring; but the northern one had ſome faint, cloudy appearance, towards the preceding, or weſtern ſide.

May 11, 1779. Two equatorial belts.

— 13, —— A bright belt over a dark one.

— 22, —— One dark, and one very faint white belt.

— 23, —— A dark belt, and a pretty bright white one.

Jan. 21, 1780. Two belts; the moſt north clouded.

— 22, —— Faint belts.

May 17, —— A dark, equatorial belt.

— 23, —— A ſtrong, equatorial belt.

June 19, at 10 h. 15′, With a new, excellent ſeven-feet ſpeculum, I ſee two belts, and a cloudy appearance, which is not come up to the middle; but, as it is a large figure, ſome part of it is already paſt the center (this is, provided Saturn turns upon its axis the ſame way as Jupiter does). See fig. 8. where the ring is omitted.

June 20, 1780. 10 h. 10′, The ſame figure is on the diſk, but ſeems to be more central than it was yeſterday.

— 21, 9 h. 25′, The ſame two belts; a ſtrong, dark ſpot, near the margin of the diſk; ſee fig. 9.; ring not expreſſed.

10 h. 1′, The ſpot not ſo remarkable as it was at 9 h. 25′.

— 26, Small twenty-feet teleſcope; an equatorial belt, and another leſs marked.

— 29, Two dark equatorial belts.

April 19, Two belts.

Auguſt 23, Two belts, a little declining from the equatorial poſition.

Auguſt

August 26, A broad belt much inclined; with 200, 250, 300, 400, faint appearances of a ſecond and of a third belt.

—— 27, The belts leſs inclined.

Sept. 2, A darkiſh belt, but very little inclined; and a fine white belt, cloſe to the ring.

— 5, The belt a little inclined.

— 6, The belt not inclined.

— 8, The bright belt cloſe to the ring, and two dark equatorial belts.

It will not be neceſſary to continue the account of theſe belts up to the preſent time; but I have conſtantly obſerved them, and found them generally in equatorial ſituations, though now and then they were otherwiſe.

We may draw two concluſions from what has been reported. The firſt, which relates to the changes in the appearance of the belts, is, that Saturn has probably a very conſiderable atmoſphere, in which theſe changes take place; juſt as the alterations in the belts of Jupiter have been ſhewn, with great probability, to be in his atmoſphere. This has alſo been confirmed by other obſervations: thus, in occultations of Saturn's ſatellites, I have found them to hang to the diſk a long while before they would vaniſh. And though we ought to make ſome allowance for the encroachment of light, whereby a ſatellite is ſeen to reach up to the diſk ſooner than it actually does, yet, without a conſiderable refraction, it could hardly be kept ſo long in view after the apparent contact. The time of hanging upon the diſk, in the ſeventh ſatellite, has actually amounted to 20 minutes. Now, as its quick motion during that interval carries it through an arch of near ſix degrees, we find, that this would denote a refraction of about two ſeconds, provided

provided the encroaching of light had no ſhare in the effect. By an obſervation of the ſixth ſatellite, the refraction of Saturn's atmoſphere amounts to nearly the ſame quantity; for this ſatellite remained about 14 or 15 minutes longer in view than it ſhould have done; and as it moves about 2¾ degrees in that time, and its orbit is larger than that of the ſeventh, the difference is inconſiderable. It is not my preſent intention to enter into a conſideration of the amount of theſe refractions, otherwiſe we might perhaps find data enough to ſubject them to ſome calculation. But what has been ſaid will ſuffice to ſhew, that very probably Saturn has an atmoſphere of a conſiderable denſity.

The next inference we may draw from the appearance of the belts on Saturn is, that this planet turns upon an axis which is perpendicular to the ring. The arrangement of the belts, during the courſe of fourteen years that I have obſerved them, has always followed the direction of the ring, which is what I have called being equatorial. Thus, as the ring opened, the belts began to advance towards the ſouth; and to ſhew an incurvature anſwering to the projection of an equatorial line, or to a parallel of the ſame. When the ring cloſed up, they returned towards the north; and are now, while the ring paſſes over the center, exactly ranging with the ſhadow of it on the body; generally one on each ſide, with a white belt cloſe to it. When I ſay, that the belts have always been equatorial, I paſs over trifling exceptions, which certainly were owing to local cauſes. The ſtep from equatorial belts to a rotation on an axis is ſo eaſy, and, in the caſe of Jupiter, ſo well aſcertained, that I ſhall not heſitate to take the ſame conſequence for granted here. But, if there could remain a doubt, the obſervations of

June 19, 20, and 21, 1780, where the ſame ſpot was ſeen in three different ſituations, would remove it completely.

There is another argument, of equal validity with the former, which now I ſhall bring on. It is founded upon the following obſervations, and will ſhew that Saturn, like Jupiter, Mars, and the Earth, is flattened at the Poles; and therefore ought to be ſuppoſed to turn on its axis.

July 22, 1776. I thought Saturn was not exactly round.

May 31, 1781. It appears as if the body of Saturn was at leaſt as much flattened as that of Jupiter; but as the ring interferes, this may be better aſcertained eight years hence.

Auguſt 18, 1787. The body of Saturn is of unequal diameters, the equatorial one being the longeſt.

Sept. 14, 1789, 23 h. 36′ 32″. Having reſerved the examination of the two diameters of Saturn to the preſent as the moſt favourable time, I meaſured them with my twenty-feet reflector, and a good parallel-wire micrometer.

Equatorial diameter,	1ſt meaſure,	21″,94
	2d . . .	23,11
	3d . . .	21,73
	4th . .	22,85
	Mean	22,81
Polar diameter,	1ſt meaſure,	20,57
	2d . . .	20,10
	3d . . .	21,16
	Mean	20,61

By this it appears that Saturn is conſiderably flattened at the poles. And as the greateſt meaſures were taken in the line of the ring and of the belts, we are aſſured that the axis of the planet is perpendicular to the plane of the ring; and that the equatorial diameter is to the polar one nearly as 11 to 10.

We may alſo infer the real diameter of Saturn from theſe meaſures, which are perhaps more to be depended upon than any that have hitherto been given. But as in my journal I have meaſures that were repeatedly taken theſe ten years paſt, not only of the diameter of Saturn, but of the ring, and its opening, whereby its inclination may be known; as well as of the diſtance of the fourth, and fifth, and other ſatellites, which will be of great uſe in aſcertaining the quantity of matter contained in the planet, I reſerve a full inveſtigation of theſe things for another opportunity; ſince, from the date of this Paper, it will be ſufficiently evident, that there can be no time for me to enter properly into the ſubject.

One beautiful obſervation of the tranſit of the ſhadow of the fourth ſatellite over the diſk of Saturn, I muſt add, to conclude this Paper.

Laſt night, November 2, 1789, at 23 h. 13′ ſidereal time, being always in queſt of any appearance that may afford the means of aſcertaining the rotation of Saturn on an axis, I diſcovered a black ſpot on the following margin of the diſk of that planet.

At 23 h. 21′, I perceived a protuberance on the ſouth preceding edge of the diſk, which I ſuppoſed to be the fourth ſatellite going to emerge.

At

At 23′, I found that the black ſpot had advanced a little towards the preceding ſide.

At 30′, with a power of 300, I found it ſtill advancing, and ſaw that the ſpot was a little to the north of the equatorial belt, but ſo that a ſmall part of it was upon the belt.

At 35′, the black ſpot was a little more than one-eighth of the diameter of Saturn advanced from the following edge towards the center.

At 39′, the ſatellite was detached.

At 49′, the ſpot was advanced ſo as to be about one-third of its way towards the center; and the fourth ſatellite near half its own apparent diameter clear of the edge.

In this ſituation of the planet I took an eye-draught of it, (ſee fig. 10.) as it appeared with the black ſpot on the belt; the lately emerged fourth ſatellite; two parallel dark belts, the intermediate ſpace between them and the equatorial one being a little brighter than the reſt of the diſk; the ſixth, third, and ſecond ſatellites on the preceding ſide; the ring projecting like two very ſlender lines on each ſide of the diſk, and containing the firſt ſatellite upon the following arm, with the fifth at a conſiderable diſtance following.

At 0 h. 5′, the black ſpot was got a little more than half way towards the center. It was much darker than the belt, and more upon it than before.

At 1 h. 2′, by advancing gradually towards the ſouth, it was now almoſt intirely drawn upon the equatorial belt.

At 1 h. 13′, the black ſpot approached towards a central ſituation.

At 1 h. 21′ 51″, it was perfectly central, and at the ſame time upon the middle of the equatorial belt.

I followed the ſhadow of the ſatellite with great attention up to the center, in order to ſecure a valuable epocha, which may ſerve to improve our tables of the mean motion of this ſatellite.

WILLIAM HERSCHEL.

Slough, near Windſor,
November 3, 1789.

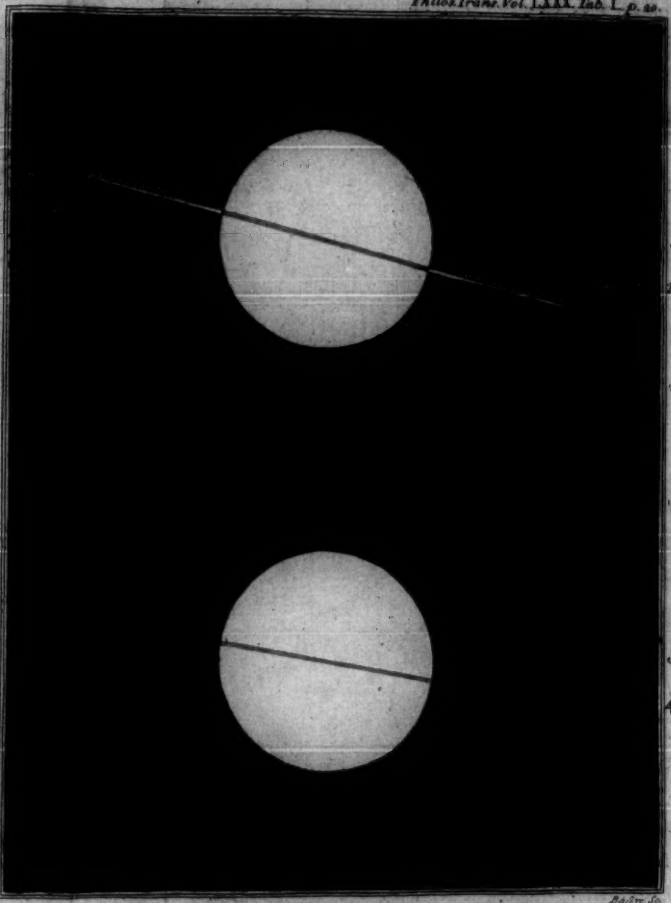


Fig. 1.

March 7. 1774

Fig. 2.

April 3. 1774.

Basire Sc.

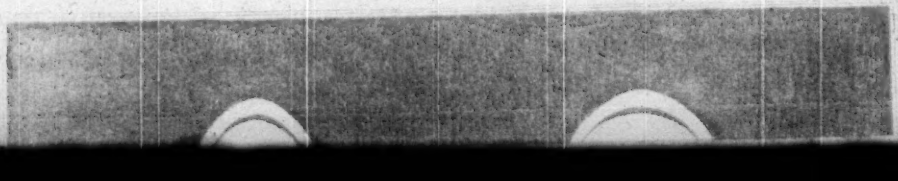


Fig. 3.

June 10th 1779

Fig. 4.

May 21st 1780.

Basire Sc.

Fig. 5.

Fig. 6.

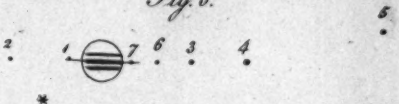


Fig. 7.

Fig. 8.

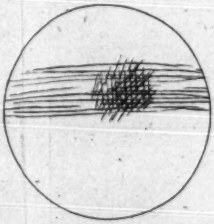
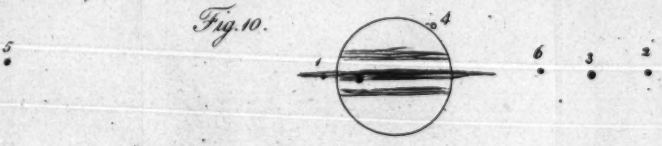


Fig. 9.

Fig. 10.



Sayer sc.

II. *Aſtronomical Obſervations on the Planets Venus and Mars, made with a View to determine the heliocentric Longitude of their Nodes, the annual Motion of the Nodes, and the greateſt Inclination of their Orbits.* By Thomas Bugge, *F. R. S. Regius Profeſſor of Aſtronomy at* Copenhagen, *Member of the Academies of* Stockholm, Copenhagen, Manheim, *and* Drontheim, *and Correſpondent of the Academy of Sciences at* Paris.

Read November 26, 1789.

I. *The heliocentric longitude and annual motion of Venus's nodes.*

THE following aſtronomical obſervations were made at the Royal Obſervatory at Copenhagen with a ſix-feet tranſit inſtrument, and with a mural quadrant of ſix-feet radius. It would be too tedious to enumerate all the original obſervations, which either are already printed in the firſt volume of my Aſtronomical Obſervations, or will very ſoon be publiſhed in the ſecond volume. I only ſhall ſet down the obſerved geocentric longitudes and latitudes, corrected for aberration and nutation, and compared with the tables of Dr. HALLEY and of M. DE LA LANDE.

Mean

Mean time at Copenhagen.	Observed geocentric longitude of ☿.	Observed geocentric latitude of ☿.	HALLEY's error,		DE LA LANDE's error,	
			in long.	in lat.	in long.	in lat.
1781 h. ′ ″	s. ° ′ ″	° ′ ″	″	″		
Sept. 13 1 38 6	6 18 39 15	0 28 12 N	+11	+ 3	″	″
22 1 43 24	6 29 38 48	0 3 9	+17	+ 1	+39	+ 3
Oct. 1 1 49 46	7 10 36 42	0 23 48 S	+16	− 7	+44	− 2
4 1 52 11	7 14 15 17	0 33 3	+12	− 4		
1784						
Sept. 20 0 37 17	6 9 40 27	1 6 15 N	+41	+15	+17	+12
25 0 39 57	6 15 52 59	0 57 50	+15	+ 3		
Oct. 2 0 44 49	6 24 35 34	0 44 21	+36	+ 6		
14 0 54 7	7 9 30 49	0 16 41	+10	+ 1		
21 1 0 42	7 18 13 11	0 1 3 S	+17	+ 1	+18	+ 3
1786						
Aug. 19 2 25 57	6 4 45 54	0 23 7 N	− 5	− 3		
20 2 26 14	6 5 56 11	0 19 40	−17	− 9		
21 2 26 32	6 7 6 42	0 16 20	−13	− 5	+17	−12
28 2 28 30	6 15 7 50	0 8 46 S	− 0	− 2	+23	+ 2
29 2 28 47	6 16 27 39	0 12 32	−22	− 4		

The angle at the planet or the commutation = P is not directly to be taken out of the table. The difference between the observed geocentric longitude of the planet and the geocentric longitude of the sun, calculated from M. MAYER's solar tables, is the angle at the earth, or the elongation = T. From this elongation, which is to be depended upon to very few seconds, and from the planet's and the earth's distances from the sun, according to the tables, the commutation is calculated, and the geocentric longitude is reduced to the heliocentric longitude. The angles P, T, and S, at the sun, thus found, are likewise used to calculate the heliocentric latitude.

According to the different dimenſions, given to the orbit of the planet in the different tables, the radius vector at a given time will alſo be ſomewhat different. Theſe differences in the tables of Dr. HALLEY and M. DE LA LANDE are but ſmall: thus, 1784, September 20, at 0 h. 37′ 17″ mean time, the obſerved and apparent geocentric longitude of Venus 6 s. 9° 39′ 54″, the aberration and nutation +33″, the corrected and true geocentric longitude 6 s. 9° 40′ 27″, the ſun's geocentric longitude 5 s. 28° 5′ 51″; hence the elongation T = 0 s. 11° 34′ 36″. If now the logarithm of the radius vector SP is taken out of Dr. HALLEY's tables = 4.858251, then $\text{ſin. } P = \frac{ST \times \text{ſin. } T}{SP}$ and P = 16° 11′ 52″; but if the logarithm SP is taken out of the tables of M. DE LA LANDE = 4,858168, the angle P will be found = 16° 12′ 4″, the difference is 12″. This uncertainty in the commutation, and conſequently in the heliocentric longitude, would have been ſtill greater if the calculations had been made only from the tables, or from the planet's geocentric longitude by the tables; thus this angle P is, according to Dr. HALLEY, = 16° 10′ 52″; and from the tables of M. DE LA LANDE = 16° 10′ 13″.

Mean

Mean time at Copenhagen.	Heliocentric longitude of ♀ in the ecliptic.	Heliocentric latitude of ♀.	HALLEY's error,		DE LA LANDE's error,	
			in long.	in lat.	in long	in lat.
1781 h. ′ ″	s. ° ′ ″	° ′ ″	″	″		
Sept. 13 1 38 6	7 28 40 26	0 56 13 N	+21	+ 5		
22 1 43 24	8 12 59 47	0 6 4	+70	+ 2	+ 63	− 6
Oct. 1 1 49 46	8 27 15 26	0 44 6 S	+41	−13	+ 13	− 6
4 1 52 11	9 1 59 21	1 0 28	−20	− 8		
1784						
Sept. 20 0 37 17	6 25 51 19	2 23 46 N	+28	+34	+106	+27
25 0 39 57	7 3 52 40	2 13 7	+ 1	+ 7		
Oct. 2 0 44 49	7 15 6 32	1 40 46	+74	+13		
14 0 54 7	8 14 13 57	0 36 58	+33	+ 1		
21 1 0 42	8 15 21 40	0 2 17 S	+55	− 2	+ 43	+ 7
1786						
Aug. 19 2 25 57	8 4 12 41	0 37 6 N	+ 25	− 4		
20 2 26 14	8 5 47 32	0 31 22	+ 52	−15		
21 2 26 32	8 7 23 10	0 25 55	+ 68	− 6	+ 69	−16
28 2 28 30	8 18 30 24	0 13 17 S	+108	− 3	+ 89	+ 3
29 2 28 47	8 20 5 16	0 18 52	+ 35	− 6		

Let the difference between two heliocentric longitudes, one before and the other after the paſſage through the node, be $= a$, the northern heliocentric latitude $= b$, and the ſouthern $= \beta$; let the arc of the ecliptic from the node to the longitude, anſwering to the ſouthern latitude, be $= x$; then tang. $x = \frac{\text{ſin. } a \text{ . tang. } \beta}{\text{tang. } b + \text{cof. } a \text{ tang. } \beta}$. By this formula the diſtance from every longitude with a ſouthern latitude to the node may be found; and hence the heliocentric longitude of the node.

Obſervations

Observations compared.	Heliocentric longitude of ♀.	Reduced to 1786.
1781	s. ° ′ ″	
September 13 — October 1	8 14 42 42	
September 13 — October 4	8 14 42 23	
September 22 — October 1	8 14 42 29	
September 22 — October 4	8 14 42 12	
Mean	8 14 42 24	s. ° ′ ″ 8 14 44 53
1784		
October 21 — September 20	8 14 43 38	
October 21 — September 25	8 14 43 40	
October 21 — October 2	8 14 43 42	
October 21 — October 4	8 14 43 30	
Mean	8 14 43 38	8 14 44 34
1786		
August 19 — August 29	8 14 45 3	
August 19 — August 28	8 14 44 28	
August 20 — August 29	8 14 44 16	
August 20 — August 28	8 14 44 0	
August 21 — August 29	8 14 44 27	
August 21 — August 28	8 14 44 36	
Mean	8 14 44 28	8 14 44 28

Hence *the heliocentric longitude of the descending node of the planet Venus was*, 1786, *August* 25, *at* 8 h. 39′ = 8 s. 14° 44′ 38″. I think that this place of the node is to be depended upon to 10 or 15 seconds. According to the tables of M. CASSINI, the longitude of the node 8 s. 14° 48′ 31″, the difference, or the error, − 3′ 53″. According to the tables of Dr. HALLEY 8 s. 14° 42′ 39″, the difference + 1′ 59″. From the tables of M. DE LA LANDE 8 s. 14° 45′ 15″, the difference only − 37″.

In order to ascertain the annual motion of the node, this observation is to be compared with the observations of other astronomers. The numbers in the column A are found by setting out from my own observations 1786. In the column B, the observation of M. DE LA LANDE 1769 is taken as the first; the series C is begun with the determination of M. HORROX 1639; and in the series D, M. CASSINI's observation 1698 is taken as the basis.

The Astronomer.	The time of observation.	Heliocentric longitude of ☊ ☿.	Annual motion of ☊ ☿.			
			A.	B.	C.	D.
		s. ° ′ ″	″	″	″	″
HORROX	1639, Dec. 4	2 13 27 50	31,3	31,6	– –	
CASSINI	1698, Sept. 4	2 14 1 45	29,2	29,2	36,5	
CASSINI	1705, June 11	2 14 2 52	30,9	31,3	31,9	
CASSINI	1731, April 7	2 14 17 2	30,0	30,2	32,7	24,9
DE LA CAILLE	1746, Dec. 21	2 14 23 10	32,1	34,3	31,0	26,8
DE LA CAILLE	1761, June 5	2 14 31 30	31,2	– –	31,3	27,3
DE LA LANDE	1769, June 3	2 14 36 26	28,1	– –	31,6	29,2
BUGGE	1786, Aug. 25	2 14 44 38	– –	– –	31,3	29,2
		Mean	30,4	31,3	32,3	27,5

If the mean be taken of those four means, *the annual motion of Venus's node will be* 30,37″, or very near 31″, adopted in the tables of Dr. HALLEY and M. DE LA LANDE.

II. *The greatest inclination of the orbit of Venus to the ecliptic.*

In the first place, I shall put down the observed geocentric longitudes and latitudes, corrected for aberration and nutation.

Mean

Mean time at Copenhagen.	Geocentric longitude of ♀.	Geocentric latitude of ♀.	Halley's error,		De la Lande's error,	
			in long.	in lat.	in long.	in lat.
	h. ′ ″	° ′ ″	° ′ ″		″	″
1781, July 20	0 1 40	4 11 10 49	1 24 46 N	+ 30	+ 5	
24	4 5 45	4 16 5 52	1 27 21	+ 21	+ 5	
30	1 11 18	4 23 28 41	1 29 21	+ 29	+ 9	+ 69 + 10
31	1 12 9	4 24 42 29	1 29 36	+ 32	+ 12	
Aug. 1	1 12 58	4 25 55 54	1 29 21	+ 18	+ 3	
4	1 15 21	4 29 37 13	1 29 4	+ 20	+ 7	
1782, July 13	21 9 19	2 10 54 17	2 12 22 S	+ 53	+ 2	+ 47 + 3
Nov. 5	22 50 43	6 29 46 25	1 21 11 N	+ 40	+ 3	+ 65 + 4
1783, Sept. 19	2 4 37	7 3 52 29	6 3 1 S	− 115	+ 39	− 58 + 24
20	2 2 2	7 4 15 51	6 10 26	− 122	+ 49	
26	1 43 58	7 5 55 9	6 50 19	− 190	+ 55	
Oct. 2	1 22 55	7 6 20 38	7 20 29	− 130	+ 58	− 24 + 44
1784, May 18	22 29 58	1 7 1 56	1 28 35 S	+ 57	+ 6	+ 44 + 5
Sept. 8	0 30 11	5 24 45 11	1 20 2 N	+ 27	+ 3	
20	0 37 17	6 9 40 27	1 6 15	+ 41	+ 15	+ 77 + 12
25	0 39 57	6 15 52 59	0 57 50	+ 15	+ 3	
1785, July 29	20 53 21	2 22 5 27	3 53 17 S	− 40	+ 13	− 13 + 19
Nov. 27	21 29 57	7 9 30 40	1 37 43 N	− 5	+ 4	+ 26 − 2
1786, June 19	1 43 7	3 21 39 32	1 30 40 N	+ 12	+ 12	
24	1 49 20	3 27 44 41	1 35 52	+ 53	+ 19	
29	1 55 7	4 3 47 42	1 38 54	+ 3	+ 5	
July 1	1 57 17	4 6 13 9	1 39 45	+ 9	+ 7	+ 43 + 8
14	2 9 7	4 21 54 24	1 37 40	− 6	+ 10	
1788, May 6	3 1 24	3 0 28 44	2 43 15 N			+ 20 + 22
7	3 2 17	3 1 28 48	2 44 22			+ 27 + 17
9	3 4 43	3 3 38 13	2 46 26			+ 39 + 14

The angle at the planet is found in the manner before mentioned. The following table contains the heliocentric longitudes and latitudes to the moments of mean time in the foregoing table. The heliocentric place of the node is ascertained with a tolerable degree of accuracy; hence the arc of the ecliptic

from the node to the circle of latitude, passing through the planet, is given $= d$; the inclination of the orbit to the ecliptic $= y$ is to be calculated by this formula, cot. $y = \frac{\text{sin. } d \times \text{sin. lat.}}{\text{tang. lat. hel.}}$.

	Heliocentric longitude of ☿ in the ecliptic.	Heliocentric latitude of ☿.	Halley's error,		de la Lande's error,		Inclination of ☿'s orbit.
			in long.	in lat.	in long.	in lat.	
	s. ° ′ ″	° ′ ″	″	″			° ′ ″
1781, July 20	5 0 0 53	3 16 55 N	+ 18	+ 12			3 23 32
24	5 6 30 59	3 21 27	+ 7	+ 7	″	″	3 23 31
30	5 16 15 59	3 23 36	+ 12	+ 21	+ 115	+ 21	3 23 41
31	5 17 53 28	3 23 30	+ 16	+ 29			3 23 49
Aug. 1	5 19 30 31	3 22 43	+ 3	+ 7			3 23 27
4	5 24 22 35	3 20 41	+ 4	+ 15			3 23 35
1782, July 13	0 4 2 37	3 11 57 S	+ 116	+ 4			3 23 26
Nov. 5	6 9 46 43	3 4 18 N	+ 40	+ 5			3 23 27
1783, Sept. 19	11 6 41 21	3 21 45 S	− 97	+ 23	− 183	+ 23	3 23 44
20	11 8 16 11	3 22 31	− 105	+ 27			3 23 48
26	11 17 45 53	3 23 30	− 175	+ 28			3 23 47
Oct. 2	11 27 16 44	3 18 56	− 117	+ 31	− 195	+ 30	3 23 49
1784, May 18	0 5 34 53	3 10 14 S	+ 71	+ 13	− 171	+ 11	3 23 34
Sept. 8	6 6 30 11	3 8 56 N	+ 18	+ 7			3 23 27
20	6 25 51 19	2 23 46	+ 28	+ 30	+ 116	+ 27	3 23 58
25	7 3 52 40	2 13 7	+ 1	+ 7			3 23 33
1785, July 29	11 15 44 9	3 23 30 S	− 68	+ 12	− 154	+ 12	3 23 32
Nov. 27	6 0 50 45	3 15 22 N	+ 1	+ 2	+ 99	− 2	3 23 22
1780, June 19	4 25 53 12	3 12 43 N	+ 1	+ 26			3 23 41
24	5 4 2 2	3 20 31	+ 44	+ 43			3 24 0
29	5 12 8 55	3 23 18	− 5	+ 11			3 23 30
July 1	5 15 23 58	3 23 33	+ 1	+ 14	+ 109	+ 14	3 23 35
14	6 0 28 2	3 9 10	− 8	+ 18			3 23 38
1788, May 6	5 16 58 34	3 23 37 N			+ 20	+ 20	3 23 46
7	5 18 36 3	3 23 12			+ 27	+ 21	3 23 28
9	5 21 50 56	3 22 4			+ 39	+ 19	3 23 38
					Mean		3 23 37,7

The

The heliocentric latitudes observed 1781 July 30, 1783 September 26, 1785 July 29, 1786 July 1, 1788 May 6, are very near the greatest latitude; the mean of the inclinations found on these days is 3° 23′ 40″,2, and very near the mean of all the observations 3° 23′ 37″,7. The inclination, or the greatest heliocentric latitude, may also be found by interpolation of the maximum amongst the observed heliocentric latitudes. This maximum is found 1781 = 3° 23′ 39″, 1783 = 3° 23′ 41″, 1786 = 3° 23′ 36″; the mean of those three maximums 3° 23′ 38″,6, which inclination may be depended upon to 1 or 2 seconds. The inclination of the orbit of Venus has been supposed in the tables of M. CASSINI, Dr. HALLEY, and M. DE LA LANDE, = 3° 23′ 20″, and the error of the tables + 18″,6.

III. *The heliocentric longitude and motion of the nodes of Mars.*

In a Paper printed in the Memoirs of the Royal Academy of Sciences at Stockholm, I have determined the *heliocentric longitude of Mars's ascending node* = 1 s. 17° 54′ 24″,2, *in the year* 1783, *December* 7, 20 h. 23′ 39″, *mean time at Copenhagen:* the error of M. CASSINI's tables − 10′ 35″, of Dr. HALLEY's tables − 23′ 27″, of M. DE LA LANDE's tables − 4′ 37″. I refer the reader to that Paper (Kongliga Svenska Vetenskaps Academiens nya Handlingar, Tom. VI. for the year 1785, p. 285—290.). The annual motion of Mars's node may be found by comparing the following observations of the longitude of the node. In the column A the numbers are going upwards from the observation 1783; in the column B the numbers are going downwards from the observation 1595.

The

The name of the astronomer.	Time of observation.	Heliocentric longitude of ☊ ☌.	Annual motion. A.	Annual motion. B.
		s. ° ′ ″	″	″
TYCHO BRAHE	1595, Oct. 28	1 16 24 33	28,7	"
CASSINI	1700, May 6	1 17 13 43	28,9	29,4
CASSINI	1721, Nov. 13	1 17 29 49	23,8	31,3
DE LA CAILLE	1747, May 14	1 17 37 11	27,5	28,9
DE LA CAILLE	1753, Nov. 4	1 17 42 5	24,6	29,6
MASKELYNE	1778, April 17	1 17 51 40	28,5	20,3
BUGGE	1783, Dec. 7	1 17 54 24		28,7
		Mean	27,0	29,5

The mean of the two series A and B will give *the most probable annual motion of Mars's node* 28″,2. In the tables of M. CASSINI the annual motion is 34″, in the tables of Dr. HALLEY 38″, and in the tables of M. DE LA LANDE 40″.

IV. *The inclination of the orbit of the planet Mars.*

Mean time at Copenhagen.	Geocentric longitude of ♂.	Geocentric latitude of ♂.	Error of the tables of M. DE LA LANDE, in long.	Error of the tables of M. DE LA LANDE, in lat.
h. ′ ″	s. ° ′ ″	° ′ ″	″	″
1788, Jan. 9 11 57 7	3 16 27 7	4 5 25N	+17	+1
10 11 51 28	3 16 3 35	4 6 10	+19	+7
11 11 45 50	3 15 40 15	4 6 44	+23	+10
26 10 24 50	3 10 43 39	4 3 59	+45	+19
Feb. 14 8 58 52	3 8 13 29	3 39 35	−8	+8
Mar. 9 7 37 35	3 11 13 55	3 1 13	−7	−4
12 7 29 7	3 11 59 16	2 56 52	+2	+7
13 7 26 19	3 12 15 21	2 55 24	+2	+9
14 7 23 35	3 12 31 44	2 53 56	+3	+8
16 7 18 10	3 13 5 38	2 50 56	−18	+7
April 6 6 27 49	3 20 29 42	2 22 46,7	−17	+8

The geocentric longitudes of Mars are corrected for aberration and nutation, and compared with M. DE LA LANDE's newest

neweſt tables, which after the laſt improvements commonly give the true place of Mars within the fourth part of a minute. The error in longitude $+17''$ ſignifies that the longitude in the tables is $17''$ too ſmall; and that thoſe $17''$ are to be added to the calculated longitude, in order to make it agree with the obſerved longitude.

Mean time at Copenhagen.	Heliocentric longitude of ♂.	Heliocentric latitude of ♂.	Error of the tables of M. DE LA LANDE, in long.	Error of the tables of M. DE LA LANDE, in lat.	Inclination of the orbit of ♂.
1788	s. ° ′ ″	° ′ ″	″	″	° ′ ″
Mar. 9 7 37 35	4 15 12 28	1 50 49,1 N	− 7	− 3	1 50 56
12 7 29 7	4 16 31 42	1 50 53,5	+ 2	− 4,5	1 50 56
13 7 26 19	4 16 58 8	1 50 56,0	+ 2	− 3	1 50 56,4
14 7 23 35	4 17 24 28	1 50 57,7	+ 3	− 1,3	1 50 57,7
16 7 18 19	4 18 16 50	1 50 56,7	− 4	− 2,3	1 50 56,7

The inclination of Mars is taken in the tables of M. CASSINI 1° 50′ 54″, and in the tables of M. DE LA LANDE and Dr. HALLEY 1° 51′ 0″.

I ſhall conclude this Paper with the oppoſition of Mars according to the foregoing obſervations. *The oppoſition of Mars to the ſun happened* 1788, *January* 7, *at* 8 h. 19′ 32″ *true time*; *the apparent geocentric longitude of Mars at that moment* = 3 s. 17° 17′ 8″, *and the geocentric latitude* = 4° 4′ 3′ N. Saturn was in oppoſition to the ſun, Auguſt 29, 20 h. 51′ 11″ true time; the apparent longitude ♄ = 11 s. 7° 31′ 34″, and latitude 1° 59′ 33″ S. The new planet was in oppoſition to the ſun January 18, 0 h. 28′ 33″ true time, the longitude = 3 s. 28° 10′ 7″, and latitude 0° 34′ 35″ N.

III. *An Account of ſome luminous Arches. In a Letter from Mr.* William Hey, *F. R. S. to the Rev.* Joſeph Prieſtley, *LL. D. F. R. S.*

Read December 14, 1786.

REV. AND DEAR SIR, Leeds, Dec. 31, 1783.

BEFORE I deſcribe the luminous arches which I have ſeen ſince the commencement of this year, I ſhall give you a ſhort account of two, which I ſaw ſome years ago, though I made no obſervations upon them which could at all illuſtrate their origin.

While I was at Buxton, in March 1774, I was called out by ſome gentlemen, about half paſt eight in the morning, to ſee a luminous arch, which appeared very beautiful in the atmoſphere. Being then indiſpoſed, I durſt not ſtay out of doors any conſiderable time to examine it, and only made the following obſervations reſpecting it. Its colour was white, inclining to yellow; its breadth in the crown apparently equal to that of the rainbow. As it approached the horizon, each leg of the arch became gradually broader. It was ſtationary while I viewed it, and free from any ſenſible coruſcations. Its direction ſeemed to be from about the N.E. to the S.W. at leaſt its eaſtern leg was inclined to the north, and its weſtern to the ſouth. Its crown, or moſt elevated part, was not far from the zenith. The evening was clear, and the ſtars

appeared bright. It continued about half an hour after it was first observed by the company.

In October 1775, I saw a similar arch at Leeds, of the same colour, breadth, and position. It began to disappear in five or six minutes after I had discovered it, without changing its situation. The manner in which it vanished was quite irregular; large patches in different parts, and of different dimensions, ceasing to be luminous, till the whole had disappeared. The evening was rather cloudy. I made no observation on the state of the wind during the appearance of either of these arches.

Having read Mr. CAVALLO's Paper in the Philosophical Transactions *, containing a description of a similar phænomenon, with some remarks upon its nature, I determined to pay a greater attention to this meteor, if I should ever happen to see it again. During the last spring it appeared so often, and with such a variety of circumstances, that I had an opportunity both of gratifying my curiosity, and fixing my judgement concerning it.

As I was travelling in the evening of the 21st of last March, betwixt eight and nine o'clock, I observed something like a bright cloud in the eastern part of the hemisphere; and suspecting that it might be of the kind above described, I looked through the glass in the back part of the chaise in which I was riding, and saw a similar appearance in the opposite part of the heavens. I immediately ordered the driver to stop, that I might make as accurate an observation as my situation would admit. I saw that the luminous bodies, which appeared in the eastern and western parts of the horizon, were connected by an arch of a fainter light; and recollecting that the best

* Vol. LXXI. p. 329.

method of aſcertaining the courſe and dimenſions of this arch would be to obſerve ſome of the principal conſtellations before which it paſſed, I made ſuch remarks as I was able; and afterwards, by the aſſiſtance of a celeſtial globe, while the circumſtances were freſh in my memory, I drew up the following account of this, and ſome other arches which I afterwards ſaw.

The arch which I ſaw firſt aroſe at E. by N. aſcending through the conſtellation Bootes, and having, with reſpect to the breadth of the arch, Arcturus in its center. Its ſouthern edge paſſed a little to the north of Caſtor in Gemini, and deſcended cloſe to the ſtar Bellatrix in the left ſhoulder of Orion. It reached the horizon in the W.S.W. point. In this courſe it paſſed about 12° to the ſouth of the zenith. Its breadth was, according to the beſt judgement I could form, about 9 or 10 degrees. It remained viſible about 10 or 12 minutes after I had firſt diſcovered it, and then vaniſhed gradually and irregularly. I obſerved no coruſcations, nor any motion in this arch.

I had ſcarcely travelled a mile farther, when another, and ſtill more beautiful, arch made its appearance. It aroſe a point or two nearer the N.E. than the former had done. Its ſouthern edge paſſed up a little to the north of the tail of the great Bear, which was then in a vertical poſition. Its northern edge appeared at firſt a little to the ſouth of the polar ſtar; but, during the continuance of the phænomenon, it gradually receded about 10 degrees to the ſouth. The arch deſcended about the W.N.W.; but neither the eaſtern nor weſtern extremities reached the horizon; each of them ending in a point gradually formed a little above the horizon. This arch might be about 10 or 12 degrees at its vertex. It continued viſible

for half an hour; and although I could not diſcover any coruſcations, or quick motion, in any part, yet the different portions of it were perpetually varying in the denſity of their light, and the whole arch, or at leaſt its vertex, made a ſlow and equable motion towards the ſouth. Where the light was the moſt denſe, the ſmaller ſtars were rendered inviſible by the arch, but ſtars of the ſecond magnitude were not totally eclipſed by it. This arch diſappeared, as the former, by patches; the light gradually becoming leſs intenſe. The colour of both theſe arches was white.

Before the latter arch had intirely diſappeared, a ſmall one, not quite ſo broad as the rainbow, aroſe from its eaſtern leg, and aſcending in a curvilineal direction to the polar ſtar, terminated there. Its light was more faint than that of the other two arches. It continued viſible about a quarter of an hour.

When I uſe the terms *aſcending* and *deſcending*, I would not be underſtood to mean, that the appearance of theſe arches was *progreſſive* from eaſt to weſt: they were all completely formed when I firſt diſcovered them; even the ſmall and imperfect arch laſt mentioned, which appeared while I was examining the larger one, had no progreſſive motion. I firſt ſaw it complete, though a few minutes before there was nothing luminous where it appeared.

The evening was very fine when I ſaw theſe beautiful phænomena; the ſtars were bright, and there was not a cloud to be ſeen except in the horizon. There was a ſteady light in the north, without the leaſt coruſcation, extending from the N.E. to N.W. The wind blew from the N.E.

On the 26th of March, about the ſame time in the evening, I was entertained with a ſimilar appearance, as I was travelling

in the country. I firſt obſerved two or three columns of *aurora borealis* ſhooting upwards in the north; and in a ſhort time after I ſaw a complete arch, like thoſe already deſcribed, though ſomewhat different in its poſition. It aroſe between the E. and N. and N.E. points, paſſed obliquely to the ſouth below Arcturus, and deſcended in the weſt through Orion, having almoſt the ſame direction through that conſtellation which the equator has. It was the moſt luminous in Orion, and juſt below Arcturus. A ſmall black cloud croſſed it in the eaſt, and hid a part of it, equal to the breadth of the cloud, from view. Its light was the moſt faint about the vertex of the arch. Its moſt denſe parts were continually varying in the intenſity of their light. The larger ſtars were viſible through its denſeſt parts. It varied its poſition, which I could beſt obſerve where it paſſed through Orion; for there it moved not leſs than 10 degrees towards the ſouthern part of that conſtellation. It continued viſible about half an hour; and, although I paid as ſtrict attention to the changes which paſſed in it as my ſituation would admit, yet I obſerved nothing which could be called a ſhooting or quick coruſcation. There was a ſteady northern light all the evening, or at leaſt till the arch had diſappeared.

The moſt grand ſpecimen of this phænomenon which I have ſeen appeared on the 12th of April, betwixt nine and ten in the evening. I had obſerved, for above half an hour, as I was travelling, a light in the weſtern part of the atmoſphere; but as this lay in the direction of ſome iron-works then before me, which often ſhoot out a flame illuminating the air to a conſiderable diſtance, I did not pay much attention to the appearance. But having paſſed the foundery, and ſtill ſeeing the light before me, I looked through the

the hind-glass of the carriage, and saw the same luminous appearance in the east. This roused my attention; and immediately letting down the glasses, I looked out and perceived a broad arch of a bright pale yellow, arising between Arcturus and Lyra, about the right leg of Hercules, and passing considerably to the south of the zenith, its northern border being a little south of Pollux, and descending to the horizon near Orion, which was then setting.

This arch seemed to me to be about 15 degrees in breadth, and was of such a varied density, that it appeared to consist of small columns of light, which had a sensible motion.

I had scarcely viewed this arch above ten minutes, when I saw innumerable bright coruscations, shooting out at right angles from its northern edge, which was concave, and elongating themselves more and more till they had nearly reached the northern horizon. As they descended, their extremities were tipped with an elegant crimson, such as is produced by the electric spark in an exhausted tube. After some time this *aurora borealis* ceased from shooting, and formed a range of beautiful yellow clouds, extending horizontally about a quarter of a circle.

The eastern leg of this arch seemed to me to make an angle of about 60 degrees with the horizon; and when I traced out the course of the arch upon the celestial globe, I judged that the center of it must have passed about 30 degrees to the south of the zenith. The crown of the arch appeared convex towards the south, and concave towards the north. The greatest part of the *aurora borealis* which darted from this arch towards the north, as well as the cloud-like and more stationary *aurora*, were so dense, that they hid the stars from view. The moon was eleven days old, and shone bright during this scene, but

did not eclipſe the brightneſs of theſe coruſcations. The wind was at north, or a little inclined to the eaſt.

The laſt phænomenon of this kind which I ſaw was on the 26th of April. Having gone into my ſtudy about a quarter before ten in the evening, when the window-ſhutters were by accident left open, I obſerved in the W. a luminous appearance, of the colour of the moſt common *aurora borealis*. From this maſs or broad column of light iſſued three luminous arches, each of which made a different angle with the horizon. That neareſt to the ſouth ſeemed to ariſe at right angles with the horizon; while that neareſt to the north made the ſmalleſt angle, and paſſed towards the N.E. through the conſtellation Auriga, having Capella cloſe to its upper edge. The houſes adjoining to my own prevented me from ſeeing the termination of any of theſe arches; and neither the time during which they remained viſible, nor the obſcurity of the atmoſphere, would permit me to trace their courſe with more accuracy. I had not viewed them many minutes when they were rendered inviſible by a general blaze of *aurora borealis*, which poſſeſſed the ſpace juſt before occupied by theſe arches.

As there was nothing peculiar in the appearance of this *aurora borealis*, except that it ſeemed to proceed from the W. where I had firſt obſerved the large column of light, I attended to the effect which the coruſcations had in obſcuring the light of the ſtars. I was ſoon ſatisfied that where the *aurora borealis* was denſe, it intirely hid from view the ſtars of the ſecond magnitude. I obſerved this particularly with reſpect to the ſtar β in the left ſhoulder of Auriga. But the coruſcations were never ſo denſe, while I ſtaid to look at them, as to render Capella inviſible. The wind was betwixt the N. and N.E. this evening.

After

After comparing the phænomena above deſcribed with each other, and with thoſe obſerved by Mr. CAVALLO, in London, March 27, 1781; by Mr. SWINTON, at Oxford, Oct. 12, 1766, and April 23, 1764; by Dr. HUXHAM, at Plymouth, Feb. 15 and 16, 1749-50; and by Mr. SPARSHAL, at Wells in Norfolk, Jan. 23, in the ſame year*; I cannot entertain a doubt, that theſe arches had all the ſame origin; and that they ought to be conſidered as a ſpecies of that kind of meteor called *aurora borealis*.

As Mr. CAVALLO has given ſome reaſons for diſſenting from this opinion, with reſpect to the arch which he deſcribes, I ſhall take the liberty of communicating my thoughts upon his arguments. His words are as follows: "This extraordinary "appearance to me ſeemed quite diſtinct from the aurora bo-"realis, for the following reaſons; *viz.* becauſe it eclipſed the "ſtars over which it paſſed; becauſe its light, or rather its "white appearance, was ſtationary, and not lambent; and "becauſe its direction was from eaſt to weſt."

1. Mr. CAVALLO certainly miſtakes in ſuppoſing that the ſtars are not eclipſed by the *aurora borealis*. The coruſcations which I ſaw on the 12th and 26th of April were more denſe than the white arches ſeen in March. The former rendered ſtars of the ſecond magnitude inviſible; but theſe I could diſcern though the white luminous arches. The *aurora borealis*, ſeen by Mr. ARDERON, at Norwich, Jan. 23, 1750, eclipſed ſtars of the firſt magnitude.

2. The ſtationary appearance of ſome of theſe arches does not, I apprehend, invalidate the opinion I have entertained concerning their nature. For the more common *aurora borealis* may now and then be obſerved to remain ſtationary for a time.

* See Philoſophical Tranſactions for theſe ſeveral years.

That

That which I ſaw on the 12th of April, and which appeared like a range of bright yellow clouds, continued fixed for a conſiderable time, though apparently formed by the combination of ſome coruſcations, which a little before had been moving with rapidity. Beſides, though there were no coruſcations obſervable in the arches which I ſaw in March, yet all of them, except the imperfect one, were perpetually varying in the intenſity of their light; a circumſtance which ſometimes takes place in the *aurora borealis*, when little or no ſhooting motion can be perceived.

Mr. SWINTON obſerved in the white arch, ſeen by him in 1764, an appearance which approaches ſtill nearer to that of the common *aurora*. "An internal undulating motion of the "the particles conſtituting the luminous matter," which "was "diſcernible from the firſt to the laſt moment of its exiſt-"ence." And Dr. HUXHAM deſcribes an appearance in the luminous arch ſeen by him, Feb. 15, 1750, which approaches ſtill nearer to that of the *aurora borealis*. "Near the top of "the arch, ſeveral very lucid, white, ſhort, vibrating co-"lumns were attached to it" at right angles, as appears by the annexed drawing, ſome of which were ſix or ſeven degrees in length. But the appearance of the arch which I ſaw the 12th of April would, in my opinion, have precluded or removed all doubt in any ſpectator with regard to the nature of this phænomenon.

The luminous arches which I have ſeen exhibited a pleaſing diverſity with reſpect to their motion; ſome of them having none other than ſuch as reſulted from a varying intenſity of their light, or an internal undulation; others moving ſlowly and equably in all their parts; and others ſeeming to conſiſt of united coruſcations, or emitting the moſt vivid flaſhes of coloured light.

3. The

3. The direction of theſe arches from eaſt to weſt ſeems to me to afford no ſufficient objection to our conſidering them as a ſpecies of *aurora borealis:* for if we ſuppoſe them to be formed by ſtreams of electric matter, flowing in a meridional direction, which are either ſtopped in the atmoſphere, or rendered viſible at a certain diſtance from the north or ſouth; this electric matter muſt acquire the form of an arch, the poſition of which will be from eaſt to weſt. This idea, reſpecting the origin of theſe luminous arches, is naturally ſuggeſted by Dr. HUXHAM's deſcription above quoted, and other of the preceding obſervations. The phænomenon of the 12th of April laſt almoſt forces this idea upon the mind, and demands aſſent.

After weighing the preceding obſervations, you will not, I hope, think me precipitate, if I venture to give a name to the meteor I have been deſcribing, and call it the ARCHED AURORA BOREALIS.

Some luminous arches of this kind, which I did not ſee, were obſerved by others in the courſe of laſt ſpring. An account of one was publiſhed in the Newcaſtle Paper, and another was ſeen at Leeds.

It is remarkable, that the greateſt part of thoſe which have been publicly noticed appeared near the equinoxes. The poles of all the complete arches which I have ſeen had a weſtern variation from the pole of the equator. The arches aſcended obliquely towards the ſouth; and in all the inſtances in which I obſerved the courſe of the wind, it blew from ſome point between the N. and N.E. That ſeen by Mr. SWINTON, in April 1764, was the reverſe of theſe in ſeveral particulars. Its pole was directed to the N.E. It aſcended obliquely to the north of the zenith, and was preceded by a wind blowing from between the N. and N.W.

Theſe circumſtances were perhaps merely accidental; though, in our preſent ſtate of imperfect knowledge reſpecting theſe meteors, it may not be amiſs to take notice of them. In one of the inſtances related by Dr. HUXHAM, the wind was N.W. by N.; in the other it was eaſt.

Before I conclude this account, already I fear too much protracted, let me hazard a conjecture reſpecting the white colour and ſtationary continuance of ſome of theſe arches. Experiments in electricity, made with what is called an *exhauſted* receiver, ſhew, that the colour and motion of the electric ſpark vary in proportion to the rarity of the air in the receiver. The more the air is rarified, the more moveable and coloured is the electric *aura* paſſing through it. On the contrary, the colour of the ſpark approaches to whiteneſs, and moves with greater difficulty, as the air is admitted. Will this obſervation ſerve in any meaſure to account for the difference in colour and motion of theſe electric arches, for ſuch I preſume to call them? May we not ſuppoſe the more coloured and tranſient coruſcations of *aurora borealis* to be made in the rarer parts of the atmoſphere, while the more white and ſtationary ones poſſeſs the denſer parts *? The whiteſt arches which I ſaw were the moſt fixed; that ſeen April 12. was the moſt coloured, and had the moſt internal motion.

I am, &c.

WILLIAM HEY.

* The luminous arch ſeen by Mr. SWINTON, Oct. 12, 1766, the edges of which were in a vertical poſition, ſeems clearly to have been formed in the lower part of the atmoſphere: for, while "the upper or exterior limb was white and "reſplendent, the lower was obſcure, and ſcarcely diſtinguiſhable from the "clouds."

IV. *Extract of a Letter from* F. J. H. Wollaſton *(dated* Sydney College, Cambridge, February 24, 1784*) to the Rev.* Francis Wollaſton, *LL.B. F.R.S.*

Read December 14, 1786.

I SEND you an account of a remarkable ſtream of light which appeared laſt night, from about 9 h. 5′ to 9 h. 25′, extending entirely acroſs the hemiſphere from W. to E. It roſe from the horizon, about 10° S. of W., near δ and γ Ceti; thence aſcended in a ſtraight line, inclining a little S. to δ and ϵ Tauri, where it made an angle with its former courſe, and proceeded nearly in a vertical circle over β Aurigæ, ϑ Urſæ majoris, by Cor Caroli to Arcturus, ſetting in the horizon about 20° N. of E. The light was ſteady, not undulating like Aurora; and as it converged towards the horizon at each end, had much the appearance which I conceive the tail of a comet muſt make whoſe nucleus is juſt in the horizon. That was particularly the caſe at the W. end, where it was brighteſt, growing gradually fainter towards the zenith; the E. part was nearly of the ſame brightneſs. The greateſt breadth of the ſtream in the zenith was about equal to the diſtance between the Pointers in Urſa major. It diſappeared gradually. When firſt I ſaw it, it did not incline ſo much

towards the S. at its W. end as afterwards; but roſe directly up from δ Ceti to the zenith. I ſhould like to know, whether this has been ſeen elſewhere, and in what direction. I remarked it, becauſe I never ſaw a ſtream extend ſo ſteadily acroſs the heavens. There was very little of Aurora in any other part of the ſky; indeed what would not have been obſerved at all, had it not been for this ſtream.

V. *An Account of a luminous Arch. In a Letter from the Rev. Mr.* B. Hutchinſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 14, 1786.

SIR, Kimbolton, Feb. 24, 1784.

LAST night (Monday 23.) at nine o'clock, a very uncommon aurora borealis appeared here. When I was called down to ſee it, it had formed a perfect, uniform ſemicircle, of the apparent breadth of half a yard, reaching like the rainbow (which it entirely reſembled, only that its colour was ſimple), from the W.S.W. horizon to that of the E.N.E. Some of the brighteſt ſtars of the Bull only juſt could be ſeen through it. The whole hemiſphere was without a cloud; calm; the wind had gone down at weſt; a ſlight froſt, after a warm thaw, was taking place; and, what was moſt extraordinary, there was no other aurora borealis in the heavens till this began to fade away, which then, however, aroſe a little, due N., but without any ſtreamers; the ring had no vibratory motion. If this phænomenon ſhould have luckily been obſerved at London or Greenwich, *data* may be had to determine its height from the earth.

With this view I ran for my quadrant, and found its zenith diſtance on the meridian ſouth 11 degrees. Kimbolton is 63 miles N.N.W. of London, latitude 52° 20′.

I have the honour to be, &c.

B. HUTCHINSON.

VI. *Extract of a Letter from* J. Franklin, *Eſq. relative to a luminous Arch. Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 14, 1786.

Blockley, Feb. 25, 1786.

I CANNOT help giving you a line to let you know, that I obſerved a very odd appearance in the heavens on Monday evening laſt, about a quarter before nine o'clock. Happening to go out of the houſe, I was very much ſurpriſed to ſee a white light, broader than a rainbow, paſs acroſs the heavens from eaſt to weſt. At firſt I thought it might be a lunar rainbow; but, after ſome moments reflection, I knew it could not be from its ſituation. I now began to obſerve its ſituation and form more minutely. It was a bright white light, about 5 degrees wide in the zenith, and gradually coming to a point both ways. The eaſtern point terminated between Arcturus and the bright ſtar in the knee of Bootes. The weſtern point came nearly to the ſtar marked *a* in the Whale's mouth. The ſouthern ſide of the light was about 5 degrees above Caſtor, paſſing eaſtward above Berenice's hair, and weſtward near Aldebaran, and through the Hyades. I obſerved it till nine o'clock. Aldebaran was ſouth of it when I firſt ſaw it; but it paſſed, and got north, before nine o'clock. At five minutes paſt nine, no more of it was to be ſeen. It gradually went off in a few minutes. The ſky was very clear from clouds, and the ſtars ſhone bright,

VII. *An Account of ſome luminous Arches. In a Letter from* Edward Pigott, *Eſq. to Sir* Henry C. Englefield, *Bart. F. R. S.*

Read November 12, 1789.

DEAR SIR, April 25, 1789.

THOUGH perhaps an account of the phænomenon of February 23. has been communicated to the Royal Society, I nevertheleſs flatter myſelf the following may be of ſome uſe, as the different appearances were obſerved with accuracy.

Being at Kenſington on the above-mentioned date, I ſaw, at nine o'clock at night, a very ſingular, luminous arch in the ſky, about 4 degrees in breadth, reſembling much a bright white cloud, drawn out in great length, or ſomething like the uncoloured northern lights, without flaſhes, but ſeemingly of a more ſubſtantial texture; the ſtars appeared very bright through it; it probably had already exiſted ſome time. At about 9 h. 7′ I noted its track thus: it was viſible very near the horizon in the N.E, paſſing between Arcturus and η Bootis, almoſt covered the cluſter of Coma Berenices, and β Geminorum, then paſſed to the ſouth of Aldebaran, over the ſtars o, g, or π Orionis, where its light was fainter, and diſappeared a few degrees lower. Though its firſt appearance was that of a beautiful regular arch, I perceived, after a few minutes, its

form had varied a little, and became rather twiſted, ſo that β π was ſometimes to the north or ſouth of its center, without being uncovered. At 9½ h. its light was much fainter, broader, and more crooked. At 9 h. 40′ its length was decreaſed, extending only as far as the Gemini's feet. It alſo had moved to the ſouth of the cluſter in Berenice, and of β π, paſſing through Cancer. Its breadth at this time was conſiderably increaſed, perhaps to more than double what it was at firſt, and its brightneſs much faded. The ſouthern ſide became flaky, having about half a dozen parts hanging down, not unlike the tails of Comets, the north ſide remaining even; it ſeemed approaching towards its diſſolution. The air was remarkably clear, with a cold and ſtrong weſterly wind; flying clouds paſſing over it intercepted its light, and conſequently the column appeared divided. The north horizon exhibited a faint aurora borealis. I imagine, if this phænomenon was well obſerved in diſtant parts, a parallax might be aſcertained ſufficiently to give us ſome idea of its elevation above the ſurface of the earth. Among the phænomena of this kind recorded in the Philoſophical Tranſactions, there are two reſembling ſo exactly the above, that they deſerve the conſideration of the learned; one was ſeen in 1734-5, the other in 1749.

Some years ago I alſo obſerved a few others, very ſimilar to that juſt deſcribed; I ſhall therefore take the liberty of adding a ſhort account of them.

At Bruſſels, March 14, 1774, at about ſeven o'clock in the evening, the ſky being very clear, there appeared an arch reſembling a bright white fog, about 8 or 9 degrees broad, tolerably well defined; the brightneſs of the ſtars it covered was diminiſhed. It roſe in the eaſtern horizon, paſſed through the conſtellations of ♓, ♈, ♉, ♊, ♋, over Aldebaran, and diſappeared

diſappeared in the Bull's back. The moſt condenſed part of it was towards the middle in ⌒; ſometimes it aſſumed this form ⌒, and ſometimes that of an arch. The phænomenon laſted about three-quarters of an hour, and ſeemed not to have gone out of the zodiac, though when it diſappeared, it was more to the ſouth than when I firſt ſaw it. The air was cold, but not froſty. Towards midnight an aurora borealis was ſeen in the north, which appeared ſomething like the phænomenon juſt mentioned; I did not ſee it.

March 15, 1774, at about 7¼ o'clock in the evening, a column of light appeared in the north, ſomething like that of yeſterday; weather very fine.

At Louvain, 1775, April 19, 9 h. 30′, at night, after a ſtorm, I ſaw a bright white line of light one or two degrees in breadth extending from N.N.E. through N. to N.W. almoſt parallel to the horizon, and elevated about 9 degrees. It was brighter in the center, and ſtars of the third and fourth magnitude which it covered were much diminiſhed in brightneſs; it ſometimes rapidly vaniſhed and re-appeared, and altogether laſted near half an hour.

Wickhill in Glouceſterſhire, 1777, Feb. 26, at about 7 h. at night, I ſaw a faint white tract of light, not unlike a foggy column, about 6 or 8 degrees in breadth. It extended from the horizon W. by S. to E. by S. paſſing over the ſtars in Orion's feet, and a very little to the north of Sirius. It ſeemed to have no motion, or to alter in brightneſs. The air was rather foggy, with a few clouds and a little wind. At about 10 o'clock a ſlight aurora borealis appeared in the north with ſtreaks, extending ſometimes to the zenith.

Theſe kinds of lights ſeem to differ from the common aurora borealis in ſeveral particulars: their light is more condenſed; they aſſume the form of an arch or column, and appear either to the north or ſouth of the zenith, though I think oftenеſt to the ſouth.

I remain, &c.

EDW. PIGOTT.

VIII. *Experiments on the Analyſis of the Heavy Inflammable Air.* *By* William Auſtin, *M. D. Fellow of the College of Phyſicians; communicated by* Charles Blagden, *M. D. Sec. R. S.*

Read December 24, 1789.

IN a Paper read before the Royal Society in the year 1788, I ſuggeſted an idea, that the heavy inflammable air is a compound of the light inflammable and phlogiſticated airs. At that time I had obſerved, that the heavy inflammable air, or at leaſt fixed air, is formed upon the decompoſition of nitrous ammoniac by heating it in cloſe veſſels; and that this air is affected by the electrical ſhock, like other elaſtic fluids into whoſe compoſition the light inflammable air enters. The concluſion which I then drew from thoſe facts ſeems to be ſupported by ſeveral ſubſequent experiments, which I now take the liberty of laying before the Royal Society. Should it hereafter be found, that the real conſtitution of the heavy inflammable air differs from what I conceive to be the reſult of the facts below recited, the facts themſelves may ſtill have their uſe, as they exhibit ſeveral properties hitherto unobſerved of the moſt extenſive compound body we know, excepting water.

Several elaſtic fluids containing the light inflammable air, as the hepatic and alkaline airs, being decompoſed by the electric ſpark, I was induced to try it on the heavy inflammable

air, as ſoon as I ſuſpected that it contained the lighter air as a conſtituent part. Agreeably to my expectation, this experiment immediately detected the light inflammable air; for ſuch an expanſion took place as could not ariſe from any other known ſubſtance. Thus the heavy inflammable air was ſometimes expanded to twice its original volume; and yet, upon examining the air ſo expanded, not a ſixth part of the whole was found to have undergone a decompoſition: for inſtance, when two meaſures and three quarters were expanded to ſix, it appeared by experiment, that nearly two meaſures and a half remained in their original ſtate.

After the inflammable air has been expanded to about double its original bulk, I do not find that it increaſes further by continuing the ſhocks. Conceiving that the progreſs of the decompoſition was impeded by the mixture of the other airs with the heavy inflammable, I paſſed the ſpark through a mixture of the heavy inflammable air and of the light inflammable air, obtained from diluted vitriolic acid and iron filings; but the expanſion ſucceeded nearly as well as when the heavy inflammable was electrified alone. This is an almoſt inſurmountable obſtacle to this mode of inveſtigation: yet it has ſuch advantages in other reſpects, the air to be analyſed being unmixed with other ſubſtances, and only in contact with the glaſs and quickſilver by which it is confined, that I determined to proſecute the ſubject in this manner as well as I could.

From this partial decompoſition of the heavy inflammable air we obtain a mixture of the two inflammable airs with phlogiſticated air; that is, of the heavy inflammable air not decompoſed, of the light inflammable air diſengaged by the ſpark, and of phlogiſticated air. How much of this phlogiſticated air pre-exiſted in the heavy inflammable air, and how much

much was diſengaged during the operation, it is not eaſy to determine. Neither are we acquainted with any ſubſtance which will ſeparate the two kinds of inflammable air by combining with the one and leaving the other: but we know that dephlogiſticated air will combine, in certain proportions, with each of them, either mixed or ſeparate; that with one of them it forms fixed air, with the other water. Therefore, by inflaming dephlogiſticated air with a mixture of theſe two airs, and obſerving the quantity of dephlogiſticated air conſumed, and the quantity of fixed air produced, we diſcover the exceſs of dephlogiſticated air conſumed above what is ſufficient for the production of the fixed air; and may conclude, that this exceſs of dephlogiſticated air has combined with light inflammable air. This concluſion is further confirmed by attending carefully to the contraction which takes place upon inflaming theſe airs, which is much greater in proportion to the quantity of fixed air produced, when a mixture of the two inflammable airs is inflamed, than when the heavy inflammable air is burnt alone. It is well known, that in all experiments of this kind, what remains after the combuſtion of the airs mixed together in due proportion, and after the ſeparation of the fixed air, is chiefly phlogiſticated air. From a conſiderable number of experiments, conducted with great care and attention to all theſe circumſtances, I have endeavoured to approximate to the quantities of the phlogiſticated and light inflammable airs diſengaged, when a given quantity of the heavy inflammable air was decompoſed. But all that can be attained to, is only an approximation to truth. The quantity of air decompoſed by this method is ſo ſmall, and the ſeparation of the different parts into which it is reſolved is attended with ſuch difficulties,

that

that an accurate analyſis of the heavy inflammable air can never be obtained in this manner.

I therefore attempted to decompoſe the heavy inflammable air by means of ſulphur, which readily unites with the light inflammable air in a condenſed ſtate, and with it forms hepatic air. Having introduced ſome ſulphur into a retort, filled with heavy inflammable air, and applied a ſufficient heat to melt and ſublime it, I found, that a conſiderable quantity of hepatic air was formed. After this air was abſorbed by water, I could not perceive that the remaining air differed from the heavy inflammable air before the operation. Sulphur mixed with powdered charcoal, upon being heated, yields hepatic air in great abundance, almoſt the whole of which is abſorbed by water. The ſmall unabſorbed reſidue, which does not exceed a hundredth part of the bulk of the whole air, appears to be phlogiſticated air.

In whatever manner the heavy inflammable air was decompoſed, whether by paſſing the electrical ſpark through it, by melting ſulphur in it, or by heating ſulphur and charcoal together, an appearance conſtantly occurred, which ſeemed to indicate, that volatile alkali is formed, whenever the heavy inflammable air is decompoſed. The circumſtance is this: a ſmall piece of paper, ſtained with any blue vegetable ſubſtance, is turned green by ſtanding in the air during any of theſe proceſſes; and this green is changed to red upon the addition of an acid. The inflammable air had been very long expoſed to water, and had no ſuch effect upon blue vegetable ſubſtances before the operation.

I have concluded theſe analytic attempts with ſeveral obſervations on the formation of fixed air from ſome ſubſtances, which conſiſt only of the light inflammable, phlogiſticated, and dephlogiſticated airs, and from others, in which theſe three

airs are combined with such matters as cannot be suspected of having any place in the composition of fixed air.

I proceed now to a detail of the experiments, upon which these observations are founded.

Exp. 1. A bent glass tube, one-third of an inch in diameter, open at both ends, being filled with, and inverted in quick-silver, 2½ measures of heavy inflammable air were thrown into it, and electrical shocks were passed through this air till it measured 4½.

Lime water being then thrown up to it was not rendered in the least degree turbid.

During the operation a thin deposit, of a whitish or ash colour, appeared upon the inside of the glass tube and quick-silver. This was a common appearance, for which I can give no reason.

The measure, made use of in this and in all the following experiments, is ¼ of an inch. The air being thrown into the tube, the length of the column of air was measured by a moveable scale thus graduated. Some objection may be made to so small a measure; but it is really unavoidable on this occasion, on account of the great difficulty of decomposing the heavy inflammable air in larger quantities. I attempted it upon a larger scale in a jar perforated with brass rods, such as is used for inflaming airs; but after having worked for many hours, in passing electrical shocks through air confined in one of these jars, it was not expanded more than one quarter of its original bulk. A piece of paper, coloured with a blue vegetable substance, was turned green by standing in the air during this operation.

It was a very tedious work to throw airs into the small tube in such quantities as could be expressed in integer numbers of the

the meaſure: I have therefore generally been contented to uſe fractional numbers, and have been ſtudious only to introduce ſuch quantities of the airs as were convenient for the experiments.

The inflammable air uſed in all theſe experiments was obtained from foliated tartar. I have alſo paſſed electric ſparks through inflammable air from pit-coal, and found that it expanded in the ſame manner. Dr. HIGGINS * has ſhewn, that 5,5 of inflammable air from foliated tartar inflamed with 7,5 of dephlogiſticated air, form 5 meaſures of fixed air. Dr. PRIESTLEY † has deduced nearly the ſame proportions of the dephlogiſticated and fixed airs, by combining the dephlogiſticated and inflammable airs in a condenſed ſtate. In the following combuſtions of theſe airs, after accounting for the phlogiſticated air in the reſidues, the quantity of fixed air produced ſeems to be equal in bulk to the inflammable air combined; and the dephlogiſticated air to be to the fixed air, or to the inflammable, in the ratio of 7 to 5; or, in other words, when 5 meaſures of fixed air are formed, it appears, that very nearly 5 of inflammable air and 7 of dephlogiſticated air have diſappeared. But in burning different airs there will be conſtant variations in the reſults, ariſing from the very different ſtates of the heavy inflammable air; and therefore, in theſe obſervations, I am obliged to follow the proportions which took place in my own experiments.

Exp. 2. Three meaſures and one-third of inflammable air were expanded to $5\frac{1}{3}$: the difference is $2\frac{1}{6}$. To this air were added $3\frac{1}{3}$ meaſures of dephlogiſticated air, which increaſed the column of air to 9 meaſures. One electrical ſpark reduced them to 4.

* HIGGINS on Acetous Acid, p. 288, 289.

† PRIESTLEY, Vol. VI. p. 27.

Lime water being then thrown up left only three meaſures of air. A ſolution of liver of ſulphur did not reduce it further. The remaining air inflamed upon being brought near a candle in the open air.

In order to account for this, it muſt be obſerved, that, before the inflammation, the airs occupied the ſpace of 9 meaſures, and were reduced by combuſtion and lime water to 3. The contraction is 6 meaſures. Of theſe the meaſure of fixed air accounts for 2,4, allowing 1 meaſure of inflammable air and 1,4 of dephlogiſticated air to produce 1 meaſure of fixed air, according to the proportion ſtated in the laſt page; 2,4 meaſures, which thus went to form fixed air, being taken from 6, which is the whole contraction, leave 3,6. If we ſuppoſe this contraction of 3,6 to have ariſen from the union of the light inflammable and dephlogiſticated air, very nearly 2,4 meaſures of the former muſt have combined with 1,2 of the latter. This explains, with a tolerable degree of exactneſs, both the contraction which takes place, the reſidue after combuſtion, and the quantity of dephlogiſticated air combined. For without any expanſion, the reſidue from $3\frac{1}{2}$ meaſures of inflammable air and $3\frac{1}{2}$ of dephlogiſticated air, after forming 1 meaſure of fixed air, would be 4,43, which exceeds the reſidue in the experiment by 1,43. Some dephlogiſticated air muſt therefore be combined, beſides what enters into the fixed air: and with what other ſubſtance but the light inflammable air could it combine, ſo as to occaſion a contraction of 3,6 meaſures?

The dephlogiſticated air being in this inſtance inſufficient to ſaturate the inflammable airs, it could not be aſcertained how much of the heavy inflammable air was decompoſed, and how much remained in its original ſtate. The two following experiments were therefore made in order to determine, in what

proportion the dephlogiſticated air is ſufficient to ſaturate this inflammable air, and what quantity of fixed air they produce when inflamed.

Exp. 3. In a large exploding jar I mixed 4¼ meaſures of heavy inflammable air with 7¼ of dephlogiſticated air. After exploſion theſe airs meaſured ſomething more than 6¼, and were reduced by lime water to rather leſs than 2¼. In this reſiduary air a candle burnt with an increaſed flame, as in dephlogiſticated air.

Thus very nearly 4 meaſures of fixed air were produced from 4¼ of heavy inflammable air.

Exp. 4. Into the ſmall bent tube, which was employed in the firſt and ſecond experiments, I introduced 3¾ meaſures of inflammable air, and 5¾ of dephlogiſticated air. Theſe were reduced by inflammation to 5¼, and by lime water to 2¼.

In this experiment, 3 meaſures of fixed air were produced from 3¾ of inflammable air.

In the third experiment, the quantity of fixed air produced is 4 meaſures very nearly. The reſiduary air is rather leſs than 2¼. If to 2¼ we add a quantity of inflammable air equal to the bulk of fixed air, that is, very nearly 4 meaſures, it will amount to 6¼ full meaſure; and if we further add 5,6, which is the quantity of dephlogiſticated air neceſſary to form 4 meaſures of fixed air, we ſhall have 11,93, which is within ſeven hundredths of a meaſure of the original quantity of the two airs.

In the fourth experiment, 3 meaſures of fixed air are produced, which require 3 of inflammable air, and 4,2 of dephlogiſticated air; theſe, added to the 2¼ meaſures of reſiduary air amount to 9,53, which is two-tenths of a meaſure more than the original quantity.

It

It appears from theſe obſervations, that the proportion of dephlogiſticated and inflammable air, in the conſtitution of fixed air, above ſtated, agrees very nearly with experiment. In one inſtance, the quantity ariſing from calculating after this proportion exceeds the real quantity by two-tenths of a meaſure; and in the other, it falls ſhort of it by ſeven hundredths of a meaſure.

It is evident, that $3\frac{3}{4}$ meaſures of this inflammable air burnt in the ſmall tube are capable of forming 3 of fixed air. The fixed air, produced by inflaming the ſame airs in the large jar, bears rather a greater proportion to the inflammable air employed; for $4\frac{1}{4}$ meaſures of it produced almoſt 4 of fixed air. I tried many experiments on theſe airs mixed together in different proportions, and only in one inſtance found the product of inflammable air greater in proportion than 4 from $4\frac{1}{4}$; which I therefore conceive to be as much as this air is capable of producing, and ſuſpect, that a ſmall error muſt have been committed in that inſtance, which gave a greater proportion of fixed air. The $\frac{1}{4}$ of a meaſure which remain, are chiefly phlogiſticated air, mixed perhaps with a very ſmall quantity of inflammable air; as will appear from the reſidues in ſome of the following experiments, which contain about that quantity of phlogiſticated air more than they could derive from the dephlogiſticated air. Thus, for inſtance, in the third experiment, the reſiduary air meaſures $2\frac{1}{4}$, the quantity of dephlogiſticated air engaged in forming the fixed air is 5,6, which added together make 8,1; but the whole dephlogiſticated air amounts only to 7,25; the difference 0,85 is nearly equal to the phlogiſticated air, which I ſuppoſe to have been mixed with the heavy inflammable air.

Exp. 5. Three meaſures of inflammable air were expanded to $6\frac{1}{4}$, then $4\frac{1}{4}$ of dephlogiſticated air were added; after

inflammation they meaſured 4¼; lime water contracted them to 2¼.

In this experiment 2¼ meaſures of fixed air were formed, which is nearly 0,28 leſs than the inflammable air uſed in this experiment is capable of producing. We have therefore only 0,28 to account for the expanſion; and we cannot ſay, that the whole of this was decompoſed, as the dephlogiſticated air, even in this experiment, was not quite ſufficient to ſaturate the two kinds of inflammable air; and therefore a ſmall quantity of heavy inflammable air might remain in the reſidue in its original ſtate.

Exp. 6. A quantity of heavy inflammable air, which meaſured between 3¼ and 3½, was expanded by about 300 electrical ſhocks to 6½ full meaſure. To theſe were added 5¼ of dephlogiſticated air. After inflammation they meaſured 4¼, and were reduced by lime water to 1⅝.

The inflammable air in this experiment was between 3¼ and 3½; we will therefore take 3,29, the arithmetical mean of thoſe numbers. The quantity of fixed air produced is leſs by 0,36 than could be produced from the ſame airs without electrifying.

It is probable, that a ſmall quantity of the heavy inflammable air may eſcape unaltered in each of theſe experiments; the following were therefore made with a ſtill greater proportion of dephlogiſticated air, and the reſidues were examined with more attention.

All the preceding experiments were made with the ſame airs; theſe being exhauſted, a freſh ſupply was procured for thoſe which follow. The purity of this dephlogiſticated air was ſuch, that one meaſure of it being mixed with 1¾ of nitrous air was reduced to 0,2 of a meaſure. The inflammable air was leſs pure than the former. In the two following experiments

experiments 4,44 measures of fixed air were produced from 5,58 of the heavy inflammable; the remaining 1,18 must have been chiefly phlogisticated air.

Exp. 7. Inflammable air $2\frac{3}{4}$ and dephlogisticated air 4,58 were inflamed in the small tube. They then measured $4\frac{1}{4}$, and were reduced by lime water to $2\frac{1}{4}$. After several bubbles of nitrous air, the residue was less than 2 by nearly $\frac{1}{4}$.

In this experiment 2,09 measures of fixed air were produced.

Exp. 8. In a large exploding jar, $2\frac{1}{6}$ measures of inflammable air, and 4,17 of dephlogisticated air, were reduced by combustion to 4,1 nearly, and then by lime water to 1,75. About a measure of nitrous air being then thrown up, the residuary air measured 1,5.

The quantity of fixed air produced in this instance is 2,35.

Exp. 9. Inflammable air $2\frac{1}{6}$ measures were expanded by about 200 electrical shocks to 5 and not quite $\frac{1}{6}$ more. To the air thus expanded, I added as much dephlogisticated air as increased the column of air to $9\frac{3}{4}$; thus the dephlogisticated air was rather more than 4,09. After combustion, they measured fully 4; and, after being exposed to lime-water, rather less than 2. Nitrous air occasioned a small contraction further.

According to the proportion stated in p. 56. $2\frac{1}{6}$ measures of this inflammable air contain 0,58 of phlogisticated air; 0,58 added to 2, which went to form fixed air, amount to 2,58. But the original quantity of inflammable air, *viz.* $2\frac{1}{6}$ exceeds 2,58 by 0,25. What becomes of the 0,25 measures of inflammable air which thus disappear? I apprehend they must have undergone a decomposition, and have been expanded to 10 times their original bulk.

The dephlogisticated air was still further increased in the following experiment.

Exp.

Exp. 10. Three meaſures of inflammable air, after 150 electrical ſhocks, became 5,1. To this was added as much dephlogiſticated air as increaſed the column of air to $10\frac{1}{2}$. After inflammation it meaſured about 4,9; and was reduced by lime water to $2\frac{1}{4}$. This reſidue was not inflammable.

The contraction upon burning theſe airs was 5,93; and the quantity of fixed air appears to be 2,15. This fixed air requires 2,15 meaſures of inflammable air and 3,01 of dephlogiſticated air. Theſe deducted from the ſum of the contraction leave 2,92, above what is ſufficient to account for the fixed air produced. Suppoſing this contraction to have been occaſioned by the union of light inflammable and dephlogiſticated air, 1,94 of the former muſt have combined with 0,97 of the latter. It is evident, that the quantity of dephlogiſticated air uſed in this experiment is more than ſufficient to combine with both kinds of inflammable air.

The fixed air produced in theſe experiments is 0,23 leſs in proportion, than was produced, when the inflammable air was not decompoſed in the ſeventh and eighth experiments. It appears by calculation, that theſe 0,23 were expanded to about ten times their bulk. It is obſervable, that the expanſion in this caſe is 2,33; and that the quantity of light inflammable air which combined with the dephlogiſticated air, is only 1,97, which is 0,40 leſs. This proves, that it is very difficult, if not impoſſible, to unite the whole of theſe airs; and that a ſmall quantity will remain diffuſed in the reſidue, unleſs the airs be much purer than I have been able to procure them; yet it is not probable, that ſo large a proportion of light inflammable air as 0,40 ſhould eſcape combuſtion, over and above what eſcapes in ſimilar circumſtances, when no light inflammable air is preſent. The addition of light inflammable

air

air to a mixture of the other airs would rather contribute to render the combuſtion more general, and the reſidue conſequently leſs; for in all combuſtions, the union of the inflammable and dephlogiſticated airs is more complete, as the proportion of phlogiſticated air mixed with them is leſſened. In general, when air does not burn in ſuch circumſtances, we preſume, that it is phlogiſticated air; and upon this principle we muſt conclude, that a conſiderable part of thoſe 0,40 was phlogiſticated air.

I proceeded to repeat this experiment with a freſh ſupply of the two airs; but the tube burſting in the laſt exploſion, a ſmaller one was uſed in exp. 12, which however could not affect the reſult of the experiment.

Exp. 11. Four meaſures and a half of inflammable air and 6½ of dephlogiſticated were reduced by inflammation to 5½; and by cauſtic alkali to 2 and a very little more. The remainder was not inflammable.

The product of fixed air is 3½ meaſures.

Exp. 12. Two meaſures and three quarters of inflammable air were expanded to 6; then, to reduce the column of air, one meaſure of dephlogiſticated air was added, and the electrical ſpark was paſſed through the two airs; afterwards 2 meaſures of dephlogiſticated air were added, and the electrical ſpark again paſſed through them; and, laſtly, 3,59 meaſures of dephlogiſticated air were thrown up, and the electrical ſpark repeatedly paſſed through this mixture of airs. After theſe exploſions the airs meaſured 5½, and after the addition of cauſtic alkali 3,83.

The product of fixed air is 1,83; which is 0,30 leſs in proportion, than was produced in exp. 11. from the ſame inflammable air not electrified.

Notwithſtanding the utmoſt attention, we are liable to a ſmall error in each of theſe experiments; and there is conſequently a ſmall variation in the reſults; but, I think, they concur ſufficiently to juſtify the following concluſions.

1. That the heavy inflammable air contains the light inflammable air in great abundance.

I apprehend this light inflammable air was, before the application of the electrical ſpark, a conſtituent part of the heavy inflammable air; becauſe, if it were contained in the heavier air not as a conſtituent part, what ſhould hinder its being burnt when the heavy inflammable air is burnt? Can it be ſuppoſed, that the heavy inflammable air ſhould contain the light inflammable air in circumſtances of combuſtion, and that the light inflammable air ſhould eſcape the fire? And if the lighter air be burnt, the ſame quantity of dephlogiſticated air would be neceſſary to ſaturate it before as after its being electrified. But it is evident from the preceding experiments, that much more dephlogiſticated air is neceſſary to ſaturate the air, after it has been expanded by the electrical ſhock, than before.

2. That no fixed air is formed during the ſeparation of the lighter air from the heavy inflammable air.

Here it ſhould be obſerved, that if the conſtitution of the heavy inflammable air depended on an union of the light inflammable and fixed airs, as ſome have ſuppoſed, we ſhould certainly diſcover the fixed air, when the other part was ſeparated from it. Or, ſhould it be conjectured, that the light inflammable air is ſeparated from water ſuſpended in the heavy inflammable air, in that caſe, would not fixed air be formed from the other conſtituent part of the water uniting with the

heavy

heavy inflammable air in conſequence of the repeated electrical ſhocks?

3. That the electrical ſhock ſeparated a ſubſtance from the heavy inflammable air, which has ſome leading characters of an alkali.

When inflammable air is decompoſed by ſulphur, or when hepatic air is made from charcoal and ſulphur, we have the ſame appearance of an alkali. That this is the volatile alkali is evident from its evaporation, when hepatic air is made from ſulphur and charcoal.

4. That the heavy inflammable air, through which the ſpark has been repeatedly paſſed, when burnt with any proportion of dephlogiſticated air, does not produce ſo much fixed air, as the ſame quantity of inflammable air not electrified.

Hence it is evident, that a part of the air is actually decompoſed by the ſpark. Hence alſo we may infer, that the decompoſed air is not reſolved into light inflammable air and charcoal, of which ſome chemiſts have ſuppoſed it to conſiſt, becauſe the charcoal would combine with dephlogiſticated air after its ſeparation from light inflammable air, and we ſhould not have ſuch a defect of fixed air..

6. That the reſidues, after inflaming the decompoſed air, are generally greater than thoſe from the air in its natural ſtate, or than can be accounted for from the mixture of the heavy inflammable and dephlogiſticated airs.

This affords a ſtrong preſumption, that phlogiſticated air is extricated from the decompoſed heavy inflammable air in a ſeparate ſtate, beſides what enters into the volatile alkali, which is formed at the ſame time. If light inflammable air only were diſengaged during the decompoſition, the reſidues would certainly not be greater after inflammation with a ſuffi-

cient quantity of dephlogiſticated air; on the contrary, if the inflammable air were increaſed in proportion in the mixture, the combuſtion would be more complete, and the reſidues leſs.

Having obſerved, that ſulphur readily combines with light inflammable air, if preſented to each other at the inſtant that the inflammable air is detached from other bodies, before its particles have receded from each other, and that hepatic air is generally formed in this manner, I introduced ſome ſulphur and heavy inflammable air into a glaſs retort, firſt filled with, and inverted in quickſilver, and applied a ſufficient heat to melt it. The heat was continued till the ſulphur was ſublimed. The melted ſulphur ſoon acquired a dark reddiſh colour; as it ſublimed, it became quite black, and every part of the retort was covered with a black cruſt. On the depending part of the retort, where the melted ſulphur lodged, and where the heat was ſtrongeſt, there remained a black mark, which could not be removed by a much greater heat than that by which the ſulphur was ſublimed. The bulk of the air was not materially altered by this operation. A little blue paper being thrown up to the air after the operation, became green. Water abſorbed about one-third of it, and acquired a ſtrongly hepatic ſmell. The inflammable air was carefully waſhed, ſo as to ſeparate from it all the hepatic air. I then mixed this inflammable air with dephlogiſticated air, and inflamed them, expecting to find a greater quantity of phlogiſticated air in the reſidue, than when the inflammable air was burnt, which had not been ſubjected to this proceſs. But the difference of the reſidue does not exceed $\frac{1}{13}$ the quantity of air decompoſed in this manner, if we may judge from the following experiment.

Experiment. Inflammable air, from which hepatic air had been made, and had been ſeparated by expoſure to water, 4¼, and dephlogiſticated air 6¾, were inflamed in a large exploding jar. After the inflammation they meaſured 6; and after being agitated with lime water 2¾. This reſidue burnt with increaſed flame.

The airs uſed in this experiment were the ſame as thoſe in the ſecond experiment. The quantity of fixed air generated is only 0,035 leſs in proportion than was produced in the third experiment from the air in its original ſtate.

The reſidue is only 0,17 more than it ſhould be by calculation, allowing the dephlogiſticated and inflammable airs to enter into fixed air in the proportion of ſeven to five.

The remaining heavy inflammable air is therefore very little altered as to its quality by this operation, though it is much leſs in quantity than can be accounted for from the production of hepatic air. For the light inflammable air in the conſtitution of hepatic air is expanded to the ſame degree as in its ſimple ſtate; and an expanſion might be expected, when the hepatic air is generated from the heavy inflammable, juſt as when the lighter air is ſeparated from the heavy by the electrical ſhock; but no expanſion is obſerved in this inſtance. I therefore ſuſpect, that, when hepatic air is formed in the heavy inflammable air, the heavy air is imperfectly decompoſed; that only a part of the light inflammable air is combined with ſulphur; and that the remaining parts are precipitated in a ſtate analogous to charcoal, and blacken the ſulphur. Upon applying heat to the ſulphur thus blackened, I have perceived an hepatic ſmell. This blackened ſulphur is not entirely diſſolved, like pure ſulphur, by being boiled in cauſtic alkali, but a black powder remains. In one inſtance, this black ſubſtance diſap-

peared after long boiling in ſtrong nitrous acid. More experiments, than it is in my power to make at preſent, are neceſſary to determine fully the nature of it.

The analogy between the heavy inflammable air and charcoal is illuſtrated by the formation of hepatic air from charcoal and ſulphur. Theſe ſubſtances, heated in a ſmall glaſs retort, yield hepatic air in great abundance. The blue vegetable colour is turned green by expoſure to this air. After hepatic air had been generated for a long time from the ſame materials, without admitting any common air into the retort, ninety-nine parts in a hundred of the air which came over laſt were abſorbed by water. The inſoluble part appeared to be phlogiſticated air. Thus ſulphur and charcoal, heated in a glaſs retort, yield hepatic air, phlogiſticated air, and volatile alkali, or a ſubſtance very analogous to it.

As far as I have been able to diſcover by experiments, the heavy inflammable air and charcoal conſiſt of the ſame elements in different proportion. The application of heat to pure charcoal confirms this opinion; for the production of heavy inflammable air from charcoal, by mere heat, is conſtantly accompanied with a production alſo of phlogiſticated air. I apprehend, that in theſe caſes the charcoal is decompoſed and reſolved into theſe two parts. Whenever charcoal, or any ſubſtance containing it, is decompoſed by heat only, the phlogiſticated and heavy inflammable airs are produced; and when the heat is intenſe, Dr. HIGGINS has obſerved *, that the air produced from theſe ſubſtances becomes rarer; as I imagine, in conſequence of a portion of the heavy inflammable air itſelf being reſolved by heat into its conſtituent parts. I would not lay much ſtreſs on the appearance of phlogiſticated air from

* HIGGINS on Acetous Acid, p. 293.

the

the compound forms of vegetable, animal, and bituminous ſubſtances, all of which yield phlogiſticated air and volatile alkali in great abundance; yet when the more ſimple modifications of the heavy inflammable air, as charcoal, vinegar, and, if Dr. PRIESTLEY is not miſtaken, fixed air, give out phlogiſticated air, when decompoſed in cloſe veſſels, I cannot but infer, that phlogiſticated air is an eſſential part of that peculiar ſubſtance which exiſts in all theſe ſtates, whether that ſubſtance be called charcoal, or the gravitating matter of heavy inflammable air.

Hence it appears, that the phlogiſticated and heavy inflammable airs combined, conſtitute charcoal; and that the mere application of heat always reſolves charcoal into theſe two ſubſtances. But the heavy inflammable air is itſelf a compound of the lighter inflammable and phlogiſticated airs. If phlogiſticated air be combined with the heavy inflammable, or, which is the ſame thing, if light inflammable air be taken from it, charcoal is re-produced; therefore, when ſulphur is melted in the heavy inflammable air, and hepatic air formed in it, the remaining parts of the heavy inflammable air return to the ſtate of charcoal. And laſtly, when ſulphur is melted in contact with charcoal, the decompoſition is complete; and the charcoal is reſolved into its ultimate particles, the phlogiſticated and light inflammable airs, with a ſmall admixture of volatile alkali.

Thus far I have proceeded in the decompoſition of the heavy inflammable air. The formation of this air, on many occaſions, confirms what has been ſaid concerning its analyſis. In the reſolution of compound bodies into their conſtituent parts, it may always be ſuſpected, that the whole is not accounted for, that ſome part may have eluded obſervation, till the very

parts we aſſign are put together, and the ſame compound is produced from them. The frequent production of fixed air, from ſubſtances generally not ſuppoſed to contain the heavy inflammable air, has lately given riſe to a new ſyſtem in chemiſtry. The author of this ſyſtem has the merit of pointing out the appearance of fixed air in almoſt all phlogiſtic proceſſes, in the combuſtion of various ſubſtances, in the reduction of metals, and in the decompoſition of acids; phænomena which cannot otherwiſe be accounted for, than by ſhewing that the ſpecific matter of charcoal is a compound body; that its component parts are preſent in all theſe proceſſes; and in ſome of them nothing elſe, if we except dephlogiſticated air.

I have already taken notice of the formation of fixed air from nitrous ammoniac, which is now well known to contain nothing, but the phlogiſticated, light inflammable, and dephlogiſticated airs. This ſalt, heated in cloſe veſſels, yields dephlogiſticated nitrous air in great abundance, mixed with a ſmall proportion of fixed air. I have often repeated this experiment with nitrous ammoniac, which indicated no trace of fixed air either with lime water, or with acids, before its decompoſition; but, when the ſalt was decompoſed by heat, I always found lime water rendered turbid by the generated air; and, upon adding an acid to the turbid lime water, have obſerved air bubbles to be produced in it.

When the three elementary airs are in a condenſed ſtate, and are ſet free from any combinations, they unite and form fixed air without the aſſiſtance of heat. Thus fixed air is generally produced when metals are diſſolved in the nitrous acid. In theſe ſolutions, the component parts of nitrous acid and the light inflammable air, being extricated at the ſame time, unite before they have acquired the aëriform ſtate, and conſtitute fixed air.

Objects

Objects are often too common or too near for our obſervation. Phlogiſticated air preſents itſelf in the decompoſition of ſo many bodies, that its appearance excites no enquiry; and it is not regarded as eſſential to the chemical conſtitution of the bodies which yield it, excepting in the inſtances of nitrous acid and volatile alkali, two ſubſtances of very ſmall extent in the ſcale of natural bodies. The calces of metals are well known to contain phlogiſticated air; yet the effect of this air on calcination in general, and how far the very different calces of the ſame metal are influenced in colour or other properties by the different proportions of phlogiſticated air, has never been conſidered. Fixed air is often formed from the calces of metals, mixed with water, or with ſome other ſubſtance containing light inflammable air *. Red precipitate mixed with iron filings yielded very pure fixed air. Braſs duſt mixed with red precipitate, likewiſe gave out fixed air, though in leſs quantity. Turbith mineral and iron filings, treated in the ſame manner, afforded much leſs fixed air than the red precipitate and iron filings. It is probable, that the turbith mineral contains leſs phlogiſticated air, than the red precipitate. The fixed air in all theſe experiments was mixed with phlogiſticated and dephlogiſticated air. Mr. KIRWAN † found, that the ſimple calx of mercury with iron filings and water produced fixed air. The ſame author alſo obſerved, that iron calcined with nitrous acid gave out, upon being heated, fixed air; and he found the production of this air renewed upon the addition of water. Dr. PRIESTLEY ‡ obtained fixed air from iron converted into ruſt by expoſure to nitrous air. In all theſe experiments the

* PRIESTLEY, VI. p. 253, 254.
† Eſſay on Phlogiſton, p. 114.
‡ Ibid. p. 52.

three

three elementary airs are preſent, and, being expelled by heat from the metals with which they were combined, unite with each other, and form fixed air. It is not material to the preſent argument, whether the light inflammable air be ſuppoſed to be furniſhed from water, or from the regulus of a metal: it is enough for our purpoſe, that none of the ſubſtances employed in theſe experiments, contain heavy inflammable air or charcoal, in ſufficient quantity to account for the fixed air produced, as Dr. PRIESTLEY * has juſtly obſerved.

The growth of plants affords a ſtrong proof of the formation of charcoal from the ſubſtances which have been aſſigned. If we may believe experiments, water and air alone are neceſſary to this natural proceſs; yet vegetation is the great ſource of charcoal or heavy inflammable air. This enquiry is ſtill in its infancy; but from the beſt experiments that have been made it ſhould ſeem, that plants grow beſt in phlogiſticated air; that they take in phlogiſticated air, and give out dephlogiſticated air. Theſe phænomena cannot be accounted for but by ſuppoſing, that water is decompoſed by growing plants; that part of its dephlogiſticated air is diſcharged into the atmoſphere; and that the other conſtituent part of water, with phlogiſticated air, is taken into the growing ſubſtance. Thus the phlogiſticated and light inflammable airs are brought together by the proceſs of vegetation.

* PRIESTLEY, VI. p. 319.

IX. *Some Account of the Strata and Volcanic Appearances in the North of* Ireland *and Weſtern Iſlands of* Scotland. *In two Letters from* Abraham Mills, *Eſq. to* John Lloyd, *Eſq. F. R. S.*

Read January 21, 1790.

LETTER I.

DEAR SIR. Fence Houſe near Macclesfield, Jan. 20, 1789.

YOUR requeſt ſhall be complied with; I will endeavour to deſcribe the Whyn Dykes in the iſland of Ilay, and give you my reaſons for ſuppoſing them to be of volcanic origin; but you muſt not expect a complete and ſcientific deſcription from me who am ſo little verſed in mineralogy, and have ſo little leiſure to purſue that uſeful and engaging ſtudy.

My engagements in the mineral line were the cauſe of my going to Ireland in 1787, to inſpect the copper mines in the county of Wickłow, whence I proceeded to the county of Antrim.

As I performed my journey through Ireland on horſeback, I availed myſelf of the opportunity that mode of travelling gave me, to note the ſtrata as I paſſed along; but ſhall not trouble you with my obſervations till I arrived in the neighbourhood of Moneymore, where I firſt perceived tumblers of lava; from hence by Maghera, Garvagh, Coleraine, Portruſh, and

to Baſh-Mills, lava is continually ſeen, either in ſolid maſſes, forming the baſis of the vegetable ſoil, or elſe in tumblers diſperſed over the ſurface. I employed two days in ſtudying the various appearances at the Giant's Cauſeway, and regretted being obliged to quit it ſo haſtily. So much has been already ſaid upon this ſpot, that I will only venture to remark, that the red ochry joints between the beds of rude lava, and the different heights at which the baſalt pillars are ſeen, give probability to the conjecture, that the whole maſs has been the produce of ſeveral ſucceſſive eruptions.

I embarked at Port Ballintrea, and after twelve hours ſailing arrived at Ilay. My object was to inſpect the lead mines which were open, and to view the other mineral veins in ſeveral parts of the iſland. It was impoſſible to do this, without at the ſame time noticing the ſingular appearance of thoſe maſſes, which run in a kind of veins in various directions, and are called Whyn Dykes. As my attention was principally engroſſed by the former, and my ſtay in the iſland very ſhort, I only curſorily remarked, that the latter had in ſome places a baſaltic appearance; but my time did not admit of a minute inquiry into their nature.

On my return from Ilay I landed at Portruſh; and, in my way to Ballycaſtle, I viewed the Giant's Cauſeway from the top of the cliffs, and was much ſtruck with ſeeing below me, in the fourth or eaſtern bay, a kind of Whyn Dyke, which ran into the ſea towards the N.N.E.

Examining the cliffs at Ballycaſtle, I found the horſes (or faults) of which there are ſeveral between the coals, were veins of lava (reſembling the Whyn Dykes of Ilay) ſtanding vertically, interſecting the various ſtrata of coal and freeſtone, and running into the ſea. The largeſt of the veins or Whyn

Dykes

Dykes is near twelve feet in breadth, and ranges N. by E. and S. by W. Your ſubſequent voyage to Raghery diſcovered that the Whyn Dykes pervade that iſland.

Returning to Dublin, through Clogh, Ballymena, Antrim, Glanevy, Moira, Banbridge, Loughbrickland, and until within a ſhort diſtance of Newry, I conſtantly ſaw tumblers of lava, and in ſome places the fixed maſs of lava, in which were fiſſures ranging N.E. and S.W.

When I reached home, my mind being ſtrongly impreſſed with the ſimilitude that ſubſiſts between the Ilay Whyn Dykes and thoſe of Ballycaſtle, which take their riſe in a country confeſſedly abounding with volcanic matter; that I might be enabled to form a better judgement of their ſubſtance when I ſhould again viſit Ilay, I repeatedly and attentively examined the Derbyſhire toadſtone in the neighbourhood of Buxton, and found it very like the ſpecimens of the Whyn Dykes, which I had brought with me from Ilay.

Early in the laſt ſummer I went into Ireland, and having ſpent ſome time at the mines in the county of Wicklow, I proceeded to Belfaſt; and a little to the northward of that town (near the ſpot where an unſucceſsful trial was ſome time ſince made for coal), I diſcovered in a bank a body of marl, running N.E. and S.W, between red and white ſand-ſtone, the whole included and ſurmounted by a kind of toadſtone and rude lava, whoſe joints had no particular direction. Above five miles north from the town, is a mountain called Cave Hill (from its containing three natural caverns); at the diſtance from which I ſaw it, the ſummit appeared baſaltic, under which is white limeſtone.

At Belfaſt I embarked for Ilay; but the wind, hanging to the northward, obliged us to tide along the Iriſh ſhore,

which, after paſſing Carrickfergus, chiefly conſiſts of ſtupendous baſalt cliffs; in which we obſerved the noted ſeven caves, which are fiſſures of various dimenſions going in to the weſtward. Farther north the cliffs are divided into horizontal beds of conſiderable thickneſs, by the intervention of a red ſubſtance, ſimilar in appearance to that at the Giant's Cauſeway; near the water's edge, and under the lava, the white limeſtone is frequently ſeen; and theſe appearances continue all the way to Red Bay. At ſome diſtance from the coaſt are the Maiden's Rocks, which, from their dark colour, I judge are alſo baſaltic. At the head of Red Bay, the mountains ſlope gradually to the ſea, have a red hue, and, I am told, iron ore is found in its vicinity. Being obliged to anchor in a little ſandy cove on the north ſide of Cuſhendun Bay, I landed, and found the beach ſkirted with tumblers of baſaltes, hornſtone, granite, and gneiſs. In a brook is a ſtring of iron ore, half an inch thick, running north between compact hornſtone ſides. At a ſmall diſtance from the beach is a large fiſſure or cavern, whoſe ſides are tinged red. Weſtward from hence are very large blocks of gneiſs; but I could not diſcover whether they compoſed the firm ſtrata, or were only tumblers: it is however to be remarked, that north from hence, at Murlogh, there is a bed of gneiſs, whoſe thickneſs is unknown; and again, four miles from Clogh (which is S.W. from hence), under a bed of white limeſtone, forty feet thick, I ſaw the upper part of a bed of gneiſs; ſo that what is ſeen here is probably the firm ſtratum. Sailing from hence, I plainly ſaw that the high broken point, which forms the N.E. point of Cuſhendun Bay, is compoſed of lava, with ſome rude appearance of pillars near the top; whilſt cloſe to the water's edge, and at ſome little diſtance in the ſea, were tumblers of an immenſe ſize.

It

It was evening when we ſailed from hence, and in the morning we were ſo far to the northward of Raghery, as to have but a very imperfect view of its baſalt cliffs, whilſt Fairhead and Ballycaſtle were totally obſcured by the hazineſs of the weather. At length, having been four days and nights in the paſſage, we landed at Loch Loudain, in the eaſt ſide of Ilay.

The volcanic country, which we exploded together in the iſland of Mull, having hitherto remained undeſcribed by any traveller, I ſhall now beg leave to remind you of what we ſaw there, and in our voyage to Staffa; which may help to confirm my opinion reſpecting the Ilay Whyn Dykes, which I ſhall reſerve for the ſubject of another Letter.

Sailing from Freeport, in the iſland of Ilay, at ten o'clock at night, of Wedneſday, July 2, 1788, we paſſed Colonſay, without being able to diſtinguiſh the ſubſtance of its ſhores; but entering the ſound of Iona, we ſaw that the rude coaſt of Mull, and the leſs elevated ſhore of Iona, was compoſed of red granite. At the landing place in Iona is laminated hornſtone; and a quarter of a mile north from the ruins of the Cathedral is a vein of coarſe red granite, two feet wide, ſtanding nearly vertical, and ranging with the hornſtone E.N.E. and W.S.W.; on the ſurface are tumblers of red granite, and ſome few of lava. About a mile N.W. from the Cathedral, and near the ſhore, is a vein, two feet wide, containing feldſpath and white mica, ranging E. and W. between granite ſides. Many of the rocks are tinged with iron, and there is ſome bog iron ore in the moſſes. In the S W. part of the iſland, is a body of white marble, veined with pale green. At the Cove, where it is ſaid St. Columb landed, the cliffs are of red granite, and the ſhore is covered with great variety of pebbles of ſerpentine, baſaltes, granite, quartz, and other ſubſtances. The

N.W.

N.W. part of the iſland is very rocky, affording little paſture, except in ſome low ſpots, where the ſoil is ſandy, and produces not only graſs, but likewiſe corn and potatoes. The whole extent of the iſland is three miles in length N.E. and S.W. and one mile in breadth; and it intirely conſiſts of alternate barren crags and little fertile vales.

Having engaged a boat with four rowers, we went from Icolmkill through the Bull Sound, which runs between Nun's Iſland and the iſland of Mull; on both ſides the cliffs are of red granite, ragged and broken, without any regular beds or fiſſures, and having no particular range or inclination. Hence we ſteered for Ardlun Head, which forms the S.W. point of Loch Leven. When we approached the Head, we ſtopped the rowers, and ſat ſome time contemplating the wonderful arrangement of the baſalt columns; and, as we again rowed along ſhore to the eaſtward, had a fine view of the various ſituations into which the columns were thrown. The coaſt being every where ſteep, it was ſome time before we could find a convenient place to land; but having at laſt got on ſhore, we walked to the extreme point or head: here, ſtruck with the errors of our maps, which placed the iſlands in ſight very differently from their true ſituations, I took the following bearings by the compaſs.

The N.E. point of Iona N.W. by W.—The Dutchman's Cap N.N.W.—Cairnborough N. by W.—Staffa from N. to N. ¼ W. diſtant, by eſtimation, about three leagues.—Rhu Thalve, the northernmoſt extreme of Mull, N. by E.—Inch Kenneth N.E. ¼ N.—The point of Ben Vawruch, on the north ſide of Loch Leven, N.E. diſtant, by eſtimation, three miles.—The range of Loch Leven E. by S. and W. by N.

About a quarter of a mile from the ſpot where the bearings were taken, is a deep glen, running N.N.E. to the ſea. It is about thirty yards in length, and twenty in breadth. The ſtrata are diſpoſed in the following extraordinary manner. The uppermoſt is ten yards of lava, with horizontal diviſions and vertical joints, taking the form of rude pillars. Under this is an horizontal bed of a perfectly vitrified ſubſtance, which appears to have been a ſhale, and is from one to two inches in thickneſs. Beneath this, is about three yards of a ſiliceous gravelly concrete; below which are horizontal beds of indurated marl, of various thickneſſes, from ſix to twelve inches. The whole of theſe beds, taken together, are about four yards, and there is a large fiſſure in them, on the weſt ſide of the glen. Laſtly, are ten yards of rude lava, containing ſpecks of quartz and mica unaltered, pieces apparently of granite, and ſome nodules of calcined chert. The whole is incumbent on regular baſalt pillars, of various dimenſions, from eighteen to ſix inches diameter, varying in the number of their ſides, ſome having five, ſome ſix, and others ſeven ſides. They are alſo as variouſly diſpoſed; thoſe on the weſtern extremity of the glen being ſtraight, and lying horizontally; whilſt of thoſe on the eaſt ſide ſome are bare, and ſtanding perpendicularly; and others, which are ſurmounted by the rude lava, are inclined and curved, as if they had taken that form in cooling from the preſſure of the incumbent weight. See Tab. IV. fig. 1. Many of the pillars are very full of bladder-holes; the articulations of the joints are cloſe, though not ſo cloſe as thoſe of the Giant's Cauſeway; but, like thoſe, their tops, where expoſed, are either concave or convex.

At the extremity of the glen is an inſulated rock, ſupported by baſalt pillars (fig. 2.), which are ſomewhat curved and inclined.

inclined. Incumbent on theſe are other pillars, lying nearly horizontal, and having a rude face of lava to the weſtward. At high-water this rock is inacceſſible without a boat; but at low-water it may be eaſily got at, by ſtepping from one tumbler to another; and on the north ſide it is not difficult to climb to the top. The bottom of the glen is covered with large tumblers of lava the whole way down to the rock, and preſents the rudeſt ſcene imaginable.

Oppoſite Ardlun Head, on the north ſide of Loch Leven, is Ben Vawruch, an high promontory, whoſe ſtrata are in horizontal beds; and the hill being of a circular figure gives it the appearance of having ſeveral terraces, with a kind of caſtle or cairn on the top.

The columnar pillars at Ardlun are more or leſs regular for an extent of near a mile and an half; and all the projecting points of Loch Leven, as far as the eye could reach, appeared to be compoſed of lava.

Amongſt the rude lava, which forms the baſis below high-water mark, are nodules of cryſtal and agate, adhering in ſmall lumps to the rocks; but, being blackened by the waſhing of the ſea, are not to be diſcovered without a very nice ſearch. Our boatmen informed us, that higher up the Loch there is a bed of coal. This we wiſhed to ſee; but, as they alſo told us, that the weather, which had for ſome days paſt been very tempeſtuous, was now favourable for landing on Staffa, we determined to avail ourſelves of the opportunity, and got into the boat, highly pleaſed with what we had ſeen; and for which pleaſure we were indebted to the hint in Dr. JOHNSON's Tour to the Hebrides.

We landed without difficulty on the eaſtern ſide of Staffa, and on an eminence, near the center of the iſland, I obſerved

the following bearings. The Dutchman's Cap N.W.—Cairnborough N. by W.—The Paps of Jura (over Mull) S. by W.

The greateſt extent of the iſland is about one mile from N.E. to S.W. and in one part not more than a quarter of a mile from S.E. to N.W. It is tolerably level, the ſhore every where ſteep, and the cliffs formed by baſalt pillars or rude lava. The uſual landing place for boats is in a ſmall cove on the N.E. ſide of the iſland; but we were aſſured, that there is no anchorage for veſſels round its whole coaſt. On the ſouth ſide, riſing from a nearly horizontal bed of reddiſh ſtone, are beautiful baſalt pillars of conſiderable height, and ſtanding vertically; at a little diſtance are others inclined, and others which are curved, very ſimilar to the ribs of a ſhip. There are three caverns amidſt the baſaltic pillars; the northernmoſt goes in to the eaſtward, how far I know not; for, though we went entirely round the iſland in our boat, the tide was too high, and the ſwell too great, to permit our entering any of the caverns without the utmoſt riſk; we therefore forbore to make ſo dangerous an attempt. One of the caverns is now uſually called Fingal's Cave; but the ſchool-maſter at Icolmkill informed us, that the Erſe name for it is Fein, which ſignifies the melodious or echoing cave. On the northern part of the iſland, and at the cove where we landed, the cliffs are of coarſe lava, without any pillars. In ſome parts of the iſland the tops of the pillars are ſtanding bare; in other parts the ſurface is formed by a rude argillaceous lava, full of bladder-holes, ſome empty, others replete with quartz cryſtals. Calcareous ſpar, pebbles of indurated clay and ſhoerl, detached pieces of zeolite, are frequently ſeen, and the vegetable ſoil is a decompoſed lava. In ſome places we met with gravel containing pebbles of baſaltes, of red granite, and of quartz,

whoſe angles were worn off, and they were become round and ſmooth. On the N.W. ſide, the cliff has lately given way; a large portion of it has fallen into the ſea, and a ſtill further part appears likely to follow it.

On the iſland are two ſprings of excellent freſh water. There were three houſes (July 5, 1788) uninhabited; and barley, oats, flax, and potatoes, growing near the center, and good graſs in ſeveral ſpots. When the crops are ripe, labourers are ſent to gather them in; after which, thirty head of cattle are ſent to winter in the iſland, which, with a ſolitary herdſman to attend them, continue till ſeed-time the enſuing ſpring.

In attempting to return to Ilay, after having got within three miles of the N.W. part of Colonſay, an heavy gale of wind came on, which obliged us to bear away, and take ſhelter in the Bull Sound. The weather continuing ſtormy, we landed, and walked to Fidden, the houſe of Lieut. Col. CAMPBELL, of the Plymouth diviſion of Marines, who received us with the utmoſt cordiality, and hoſpitably entertained us for five days, which we were detained by an adverſe wind and ſeverity of weather. Availing ourſelves of this delay, and of Col. CAMPBELL's polite offer to be our guide, we ſet out on foot with him and Lieut. STEWART, to view the ſeam of coal on the ſouth ſide of Loch Leven. After paſſing ſome moory ground, and continuing our route to the northward for an hour and an half, we came to a ſpot where the rock which puts up to the day is a laminated micaceous ſtone of the gneiſs kind (KIRWAN, p. 102.); and in another half hour, near a ſmall town on the S.W. ſide of Loch Lyne, we were ſhewn a quarry where the gneiſs ranges N.E. and S.W. and has a ſlight hade to the S.E. It is in ribs from two to twelve inches thick,

and

and is ſtratified by intermediate ribs of red granite of about an inch thick. We croſſed Loch Lyne in a boat, and landed on a rude maſs of lava, which continues away to the N. E. by the town of Ardlun, and onwards to the S.W. ſide of Loch Leven.

In a ſmall bay, about one mile to the S.E. of Ardlun Head, under a bed of jointed lava, which has ſome reſemblance of pillars, and juſt at high-water mark, is a bed of coal, exactly twelve inches thick, intermixed with ſhale or bituminous ſhiſtus (KIRWAN, p. 89.) dipping S.E. towards the Loch one yard in three: there is not any intervening ſubſtance between the coal and ſuperincumbent lava, which contains many bladder holes. Beneath the coal is alſo lava without any intervening matter. About twenty yards to the N.W. the coal again appears in the cliff, but is not more than from eight to ten inches thick. Here are tumblers of various ſizes, ſcattered on the ſhore. Amongſt them are ſome reſembling the Derbyſhire toadſtone; and a ſhort diſtance inland (to the S.W.) are rude maſſes of lava, ſtanding up at day, not unlike the great Whyn Dykes of Ilay. In the Loch, and at ſome diſtance from the oppoſite ſhore, there ſtood, within the memory of man, an inſulated pillar of coal, from which the country people were accuſtomed to procure a ſupply for ſmiths uſe; but the quantities they carried away, and the continual waſhing of the ſea, have now entirely removed it.

We returned to Fidden Houſe, which is ſituated on level ground near the ſea, and near a ſmall Loch, which affords an harbour to ſmall veſſels, but is dangerous to enter. The flat country in front of the houſe is entirely compoſed of ſea ſand, including ſhells: when dug into, freſh water is conſtantly found at a few feet below the ſurface. At the back of the houſe, towards the ſea, are cliffs of red granite, which extend round to the

Bull Sound. On the ſhore are pebbles of baſaltes, of granite, and of gneiſs; and from the firm rocks we collected ſeveral ſpecimens of granite, in which the feldſpath, quartz, and black mica, are differently blended, but without any ſhoerl.

During our ſtay at Fidden, I learned from Mr. STEWART, that Rhos Mull, which is the N.W. part of Mull, is chiefly red granite: in the ſouthern part of the iſland is very fine white freeſtone, and between that and the granite all whyn-ſtone. The iſland of Liſmore, in the ſound of Mull, is intirely limeſtone, excepting where it is croſſed by the Whyn Dykes. In the iſland of Ulva are pillars ſomewhat reſembling thoſe of Staffa, but of a paler colour.—Canna alſo is baſaltic, and reſembles Staffa.—The Dutchman's Cap has rude pillars — Cairnborough the ſame. Dunvegan in the iſle of Skye has baſaltic pillars, ſimilar to Staffa.—On the ſouth-weſt ſide of the iſle of Egg is a curious cavern.

We again embarked for Ilay; but, it being calm, and the tide againſt us, were obliged to anchor; and we landed on an iſland which forms the S.E. point of the ſound of Iona. From the point, which is a bare rock of red granite, broken and jointed in every direction, I obſerved the following bearings. Icolm-kilm Church N.N.E.—The northernmoſt part of Staffa, over the N.E. point of the ſound of Iona, N.E. by N.—The ſouth Pap of Jura, over Colonſay, S.

The upper ſurface of the granite, even in the very higheſt part, is all convex, which ſeems to prove, that by ſome convulſion it has been thrown up from the bed of the ocean, which, by long waſhing over it, had previouſly worn down its ſubſtance at the edges of all its numerous joints. On the eaſt ſide of the point, and on the weſt ſide of a little bay, where the granite cliffs are at leaſt fifteen yards perpendicular, we

diſcovered a Whyn Dyke, or vein of lava*, about two feet wide, included in a vertical fiſſure ranging S.E. by E. and N.W. by W. Going round to the oppoſite ſide of the bay, we found the lava on the cliff ranging as above; but the vein, or Whyn Dyke, much ſmaller, being only from eight to ten inches between the granite ſides, which ſeemed to continue cloſing; nor could it be ſeen on the main land of Mull, which was at no great diſtance. The fiſſure, which includes the lava, is, where firſt diſcovered, wider than the vein of lava it contains, which therefore ſtands wholly detached from the S.W. ſide. The lava and fiſſure range quite through the point; and to the N.W. by W, on the oppoſite ſide of a ſmall ſound or inlet, it appears on a rocky iſland, divided into two veins, ſtill keeping nearly the ſame direction.

About ſix yards to the weſtward of the lava vein, or Whyn Dyke, is an immenſe fiſſure in the granite, ranging N. by W. and S. by E. It is from nine to ten feet wide, and, by eſtimation, about an hundred and twenty feet deep. At the northern extremity, near the top, two ſtones are ſuſpended in a moſt extraordinary manner between the ſides: the under one is fixed, and upon that the other appears to lie looſe. (See fig. 3.) There is a large cavern in the weſtern ſide of the fiſſure, and a correſponding fiſſure is ſeen on the oppoſite ſhore.

In the evening, when the tide favoured us, we ſailed. The night proved calm and foggy, and in the morning we found ourſelves near the weſt coaſt of Jura, to the northward of Loch Tarbut. As we approached the ſhore, we found it rocky for the diſtance of a quarter of a mile from the cliffs, which are low, and apparently of chert. Being quite calm, we rowed along ſhore,

* See MAGELLAN's Cronſtedt, p. 916.

paſſed

paſſed the entrance of Loch Tarbut, and obſerved ſeveral Whyn Dykes, or lava veins, running into the ſea. The Paps of Jura were moſtly covered by the fog; but which breaking away at intervals gave us a view of their lofty ſummits, and of the narrow ſtripe of rock mentioned by Mr. PENNANT (Voyage in 1772, p. 217.), called the ſlide of the Old Hag, to which is annexed a curious legend; but which, in reality, appears to be the ſurface of a Whyn Dyke, running down the ſide of the cherty Mountain. After entering the ſound of Ilay, obſerving a very conſiderable Whyn Dyke, or vein of lava, on the Jura ſhore, we landed, and found it range N.N.W. and S.S.E. It is of a dark colour and compact texture, and in ſome parts will give fire with ſteel. Near to this Whyn Dyke is a red argillaceous ſubſtance, ſtrongly impregnated with iron, and contained in an inclined ſtratum, varying in thickneſs, and terminating in a cavern, whoſe ſides and roof are of the ſame ſubſtance, but wonderfully broken and thrown into every direction, as is the including chert, amongſt which we found ſome that is brecciated.

Soon after we returned to our boat, a ſquall of wind came on, attended with heavy rain, which almoſt wholly obſcured each ſhore, and continued till we landed at Freeport, wet and fatigued, but highly gratified by the recollection of the many curious objects we had viſited during our excurſion.

It having been conſidered as very extraordinary, that a bed of coal ſhould be found, as at Ardlun, incumbent upon, and ſurmounted by, a maſs of lava; I was induced to look into ſome of the authors who have treated of volcanic countries; and find there are many inſtances of coal in the vicinity of lavas, though I have not met with any preciſely ſituated like the coal at Ardlun.

May

May not the coal found at Ardlun be an indurated bitumen, which, exuding in a liquid ſtate from the incumbent matter, penetrated the argillaceous ſhiſtus, which previouſly conſtituted the intervening ſtratum between the lava? It has ſome of the properties of jet; the ſpecific gravity of that which we procured is 1,284; it is of a gloſſy black, its fracture glaſſy and conchoidal, does not ſoil the fingers when handled, and when warmed by friction will attract light bodies. Placed on a red-hot iron it decrepitates, emits a denſe ſmoke which has a reſinous ſmell, becomes thoroughly ignited, burſts into flame, and yields an impalpable reſiduum, which is not attracted by the magnet, and of which I only procured half a grain, of a yellowiſh brown colour, from twenty grains of the crude ſubſtance.

The learned Biſhop of LANDAFF, in the third volume of his Chemical Eſſays, in his Eſſay on Bitumens (p. 6.), ſuppoſes that, under certain circumſtances, naphtha, petroleum, and aſphaltum, might be produced by a kind of ſubterraneous diſtillation, and might impregnate the porous ſtrata of ſeveral kinds of ſtones and earth. Conſider then, whether the ſubſtance I have been deſcribing may not have been produced in that manner, ſince it is included within a maſs of matter which carries every appearance of having formerly been acted upon by fire.

I am, &c.

A. MILLS.

P.S. If you compare the bearings which I have taken with the map in Mr. PENNANT's Voyage in 1772, they will be found to agree; and if the variation of the needle be allowed, they will coincide with the map in Dr. ANDERSON's Preſent State of the Hebrides.

LETTER II.

DEAR SIR, Fence Houſe, Feb. 1, 1789.

HAVING in my former letter ſhewn, that Whyn Dykes, or, in other words, veins of lava, are found in the vicinity of columnar baſaltes, which latter are now, by almoſt univerſal conſent, acknowledged to be of volcanic origin; I ſhall proceed to deſcribe the Whyn Dykes of Ilay, and ſhall commence with giving you a general idea of the extent and various ſtrata of that iſland.

Ilay, from the northern to the ſouthern extremes, is about thirty miles in length, and in one part extends nearly as much in breadth from the eaſtern to the weſtern ſhores. The S.W. part of the iſland is low; but the land riſes to the N.E. and the mountains and cliffs which form the north-eaſt coaſt are ſteep and lofty. At the head of Loch-in-Daal, and near Bomore, the rock which appears at day is of curled hornſtone, and does not ſeem to have any particular direction. Purſuing the road (N.E.) to Port Aſkeg, the hornſtone becomes more regularly laminated, and ranges N.W. and S.E.; and ſome of the hollows between the little riſing hills are found to contain gravel. At four miles from Bomore commences the limeſtone ſtratum, the joints in which are nearly vertical, and range in general N.E. and S.W. It is about four miles in breadth from S.E. to N.W.; but I have not been able to diſcover how far it extends in the courſe of its range, never having traced it more than three miles in that direction; but it is nearly certain, that it does not reach either the eaſtern or the weſtern ſhore, from

from the Mull of Kinhouth, the eaſtern ſide, being hornſtone till near M‘ Arthur’s Head; and from thence, round to the weſtward, the cliffs being of chert. Continuing the route for almoſt a mile along the high road, after leaving the limeſtone, veins of ſhale, and of a whitiſh ſtone marl, are ſeen; then the chert appears, which reaches to Port Aſkeg, and north and ſouth from thence along the ſhores of the ſound not only on the Ilay but likewiſe on the Jura ſide. Within the iſland are ſeveral ſmall lochs (or lakes) and many turf moſſes. Where the limeſtone prevails are ſome minute ſtrings of rich copper ore, and mines are opened in lead ore veins; but they are not productive of profit to the adventurers. There are many old workings, ſuppoſed to have been opened by the Danes. Iron ore is found ſtratified in the mountains. Bog iron ore is met with in the moſſes; and near Bally-echra is a body of form iron ore. At Loch Knock, near M‘Arthur’s Head, is an extenſive bed of gneiſs; and near Ardleſtree is an immenſe Whyn Dyke, and the hill Knock Reneſtle is a magnetic maſs of rock, which conſiderably deranges the compaſs, as you diſcovered when you viſited it laſt ſummer.

The Whyn Dykes are too ſingular in their formation to eſcape the eye of the naturaliſt who traverſes this iſland. They are maſſes, or rather veins, generally of a dark-brown (apparently baſaltic) matter, not unfrequently containing bladder-holes; from three, four, and ſix feet, to eight or more yards in breadth, running in various directions. In ſome places they are ſtraight for a conſiderable length; in others, their courſe, though progreſſive, is inflected; and in ſome parts they riſe between three and four feet above the ſurface, forming natural boundaries or dykes (from whence their name ſeems to be

derived) ſtanding vertically, and appearing to fill up the chaſms formed at ſome remote period in the ſtrata.

In giving a more minute account of the Whyn Dykes which have fallen under my obſervation, it becomes neceſſary to mention, that when I arrived at Loch Loudain, I there hired a row boat, in which I proceeded to Freeport Bay. At the place where I re-imbarked, the ſhore is low, and compoſed of horn-ſtone. After paſſing the ruins of an ancient caſtle, in about an hour and an half I landed on a rocky point, where I diſcovered a large Whyn Dyke, between three and four yards wide, ſtanding vertically, and ranging S.W. into the land. It is of a dark-brown colour, externally approaching to black, and contains bladder-holes, ſome of which are filled with quartz, and others with reddiſh lamellar ſpar, which effervеſces ſtrongly with nitrous acid, as do many parts of the baſis of the ſubſtance, which is too ſoft to ſtrike fire with ſteel, but is conſiderably magnetic, and of the ſpecific gravity of 2,863. Embarking from hence, and croſſing a ſmall ſandy bay (named THUROT's Bay, from that gallant Frenchman having landed there), we reached the high mountainous point called M'Arthur's Head, which forms the S.E. point of the ſound of Ilay. The ſtratum here is white chert, with many large patches and ſtripes of red, which indicate that it contains iron; but the great height, and almoſt perpendicularity, of the cliffs, prevented my minutely examining them. From thence to Port-aſkeg and Freeport Bay, the rocky ſhore, which is compoſed of chert, is leſs elevated, ſhews many indications of iron, and has ſeveral fiſſures and caverns; and in ſome places has Whyn Dykes burſting through it, which appear to re-enter on the oppoſite cherty ſhore of Jura.

About

About two hundred yards north from Freeport Lodge is a Whyn Dyke, which ranges N.W. and S.E., and appears in that direction on the Jura ſhore, which is more than a mile diſtant. This Whyn Dyke is bare at the cliffs ſeveral yards in height, and is near nine feet in width. It conſiſts of an inner part of a granular and ſomewhat porous texture, of a dark grey colour, with ſhining parts like mica, includes opaque zeolite ſpecks, and its ſpecific gravity is 2,811. On each ſide of this, and divided from it by ſmall vertical fiſſures or joints, not more than an eighth, or at the moſt a quarter, of an inch wide, are two bodies, each near eighteen inches in width, of a dark colour, much reſembling the preceding ſubſtance, but without the zeolite ſpecks; the ſpecific gravity is 2,850. Both the outer and inner ſubſtances ſlightly effervеſce with the nitrous acid; both are magnetic, and both give fire with ſteel. The whole maſs is interſected, at various diſtances, by lateral joints, and is included between the chert rock, which does not appear the leaſt altered where it comes in contact with the Whyn Dyke. Farther along the ſhore is a cavern in the chert; the entrance is low and narrow, but within it is very capacious.

Intending to viſit the cave Ea mawr, on the weſtern ſide of the iſland, I took boat at Freeport, and rowing along ſhore, which is of chert, I obſerved ſeveral fiſſures or caverns in the cliffs, and many Whyn Dykes, ſome ranging N.W., others N.E. I landed, within the ſound, on a white ſhingly beach, the ſtones of which are all chert, rounded by attrition, and the ſhingle is thrown up ſo much above the preſent high-water mark, that it affords ſtrong reaſon to believe, that the ſea has greatly receded from theſe ſhores. Above the beach, the cliffs are of chert. From hence I walked three miles over moors and open paſtures acroſs the north-weſt point of the iſland, and in ſome

of the hollows between the hills met with beds of ſtones, ſimilar to thoſe I had ſeen on the beach. Approaching the weſtern ſhore, I ſaw a cavern, whoſe mouth opened to the eaſtward, and whoſe bottom was covered with water. Paſſing this, I arrived at a narrow ſteep path, which leads down the cliffs to the ſhore. Here are ſeveral caverns, whoſe general range is S.E. into the cliffs, which are compoſed of chert. South from theſe caverns, in traverſing the ſandy ſhore, I paſſed a vein of laminated hornſtone (approaching to the nature of a ſhiſtus) containing pyrites in the joints of the ſtone, which range nearly N.N.W. and S.S.E. Farther ſouth is an immenſe Whyn Dyke, burſting from the cliffs; it ſtands vertically, is many yards in heighth, projects from the cliffs to the northweſtward, and in that direction runs many fathoms into the ſea. It bears the buffeting of the waves of the Atlantic Ocean from the ſouth-weſt, and ſeems to defy their rage, though its breadth, compared with its height and length, is very inconſiderable, it not being more than five or ſix yards wide. It is of a dark granular ſubſtance, very ſimilar to the Whyn Dyke near Freeport, excepting that the central part is ſofter and of a paler colour. The outer ſides, which are each about two feet thick, are of a very dark colour, hard, contain ſome bladder-holes and ſpecks of zeolite, are detached from the center (in general) by very ſmall joints, and the whole is divided by tranſverſe joints into irregular polygons of various dimenſions. If this ſtupendous object is viewed from the north, it has much the appearance of a lofty wall of human fabrication.

A ſmall diſtance more to the ſouthward is the great cave, in the Erſe dialect called Ea mawr. The entrance is near twenty-three yards wide, and from ſix to eight yards high. After going in a little way the roof riſes, and the cavern extends in

breadth; but at about an hundred and fifty yards from the entrance, all its dimenſions are contracted, and it becomes ſo ſmall as barely to admit further progreſs without crawling on hands and knees. There are ſome calcareous ſtalactites pendent from the roof; and in this cave, as well as thoſe before mentioned, wherever the water pervades through the joints of the chert, it tinges the ſides of a ferruginous hue.

From hence I returned by the ſame route to the boat and to Freeport; and having given you an account of the principal Whyn Dykes which I examined on the ſea coaſt, I will proceed to relate thoſe obſervations which occurred in my progreſs through the interior parts of the iſland, confining my remarks more particularly to the Whyn Dykes which conſtitute the ſubject of preſent conſideration.

Aſcending the hill from Freeport, in the way to Shinegart, the road goes for about a mile over chert. Near Perſabus are beds of ſtone marl, and a very large Whyn Dyke, ranging N.E. and S.W. Hence the path leads over hornſtone; and in one part is a laminated hornſtone, ranging E. and W. It inclines to a ſhiſtus; but where it ſtands bare at day, is ſoft and partly decompoſed; but probably might be found uſeful for coverings, if it was tried in depth. Again the chert is ſeen, and again hornſtone, in which is included the lead vein of Shinegart, ſituated on the S.E. ſide of Portnealin Loch. This vein had been formerly worked; but, when the old workings were cleared out, was found not worth purſuing. The vein ranges N.N.E. and S.S.W., is two feet wide, and hades to the eaſtward, and is croſſed at right angles (a few yards to the northward of the workings) by a ſmall Whyn Dyke. At Mullrees are old workings on a lead vein, which is about four feet wide, but left poor, and ranges N. and S. between lime-

ſtone

ſtone ſides, hading to the weſtward. South-ſouth-weſt from hence is a natural cavity in the limeſtone, called the Giant's Hole. Here the joints of the limeſtone range S.E. and N.W. and are croſſed by a ſhale ſtring, three feet wide, ranging N.E. and S.W. The water of a little ſtream, which falls into the Giant's Hole, paſſes through a fiſſure in the limeſtone, and iſſues again to the day in lower ground, fifty yards to the ſouth-eaſtward. Returned by Perſabus to Freeport, having gone over ſeveral Whyn Dykes of various widths, and running in various directions.

From Freeport went (by Perſabus) to Gartneſs lead mine, which is ſituated in the center of the limeſtone country; much work has been done here, but at preſent there are no great proſpects of ſucceſs. The principal vein, which in ſome parts is about one foot wide, has a ſlight hade to the north-eaſtward, and ranges ſouth-eaſt and north-weſt, along the ſide of an immenſe Whyn Dyke, between which and the limeſtone it is included, the points of the latter running north-eaſt and ſouth-weſt at right-angles with the vein and Whyn Dyke.

This Whyn Dyke is the largeſt I have yet ſeen, being twenty-three yards wide. Where cut through, from Abel's to Hodgſon's ſhafts, it divides the limeſtone ſtrata, and is nearly vertical, having, like the vein, a very ſlight hade to the north-eaſtward. It is chiefly compact, but not entirely without fiſſures. It varies both in colour and texture; in ſome parts approaching to the texture and hardneſs of baſaltes; in others, being granular, of a black iron-grey colour with ſhining parts, gives fire with ſteel, is ſlightly magnetic, does not effervesce with nitrous acid, and its ſpecific gravity is 2,631. Other parts, of nearly the ſame texture and colour, have none of the preceding qualities; ſpecific gravity 2,698. Some

pieces are found which contain minute ſpecks of ſhoerl, and of a calcareous ſpar; ſpecific gravity 2,484. In ſome parts it is of a whitiſh grey calcareous ſubſtance, ſpecific gravity 2,542; in others of a pale greeniſh grey, whoſe ſpecific gravity is 2,322, and which does not effervefce with nitrous acid, but contains threads of calcareous ſpar, and is ſometimes alſo found coated, and ſometimes ſpangled, with pyrites, and then its ſpecific gravity is increaſed to 2,833. Throughout this great maſs, nodules of a yellowiſh grey colour are found, which do not effervefce with acids, or give fire with ſteel, and which are in appearance very ſimilar to ſome kinds of Derbyſhire toadſtone; ſpecific gravity 2,652. Other nodules are alſo met with, of a kind of indurated clay, coated like the geodes, and tinged with iron. The central part, which is the ſofteſt, after being drawn up, and lying ſome time expoſed to the action of the air, decompoſes, and moulders into a friable earth, ſimilar to that which is found on the ſurface near Belfaſt, and various other places in the county of Antrim, and in the iſland of Staffa.

Going from Gartneſs to the lead mines at Ardachie, I paſſed Glaſgow Beg, where is a lead vein, formerly wrought, ranging between limeſtone ſides E.S.E and W.N.W.: a ſmall Whyn Dyke croſſes this vein, running nearly S.S.W. and N.N.E. Farther ſouth is an old open caſt in a lead vein, ranging E. and W., and hading S. At ſome diſtance from hence is Ardachie mine. This vein, which has formerly been much wrought, has the ſame range and hade as the preceding, and is croſſed by a Whyn Dyke, ten feet wide, which throws the vein nine feet from its general direction. Proceeding from hence over moory ground, near Allala, ſaw a dead vein of

ſpar,

ſpar, limeſtone, and an argillaceous ſubſtance mixed with mica; the whole tinged with iron. Continuing my walk, I arrived at the foot of Ben Vinkie, which is compoſed of a granulated whitiſh chert. This mountain is ſaid to be the higheſt in Ilay. It is very ſteep on the S.W. ſide, where I aſcended it by following the courſe of a Whyn Dyke, which reaches very near to the ſummit. This Whyn Dyke is ſimilar to thoſe before mentioned. It conſiſts of a dark-grey granular ſubſtance, which does not effervesce with nitrous acid, but gives fire with ſteel, is ſlightly magnetic, and its ſpecific gravity is 2,901. It conſiſts alſo of a very dark granular ſubſtance, with the ſame properties as the foregoing; and further, in ſome parts, it has a granular ſubſtance, which includes ſmall pieces of white chert; other parts are of an argillaceous ſubſtance, ſomewhat reſembling hornſtone, and others of a calcareous ſubſtance which ſlightly effervesces with nitrous acid. Here is alſo a kind of vein, containing a browniſh argillaceous ſubſtance, and reddiſh cubical calcareous ſpar. The very ſummit of the mountain is compoſed of white cryſtalline chert, which is not unfrequently tinged with iron.

Loſſit Hill being an object of great curioſity, I ſhall with pleaſure retrace our walk from Gartneſs by Glaſgow Beg to Loſſit Loch and to the Hill. Walking on the weſtern ſhore of the Loch we ſaw a vein of blue ſhiſtus, many yards wide, ranging N.E. and S.W.; not far from which is an ochraceous earth, and much bog iron ore. Obſerving, on the ſouth ſide of the Loch, the appearance of an ochry earth, we went round to examine it; and near it we found an immenſe Whyn Dyke, ranging nearly S.E. and N.W. compoſed of a friable ſubſtance containing zeolite, of a black granular ſubſtance which gives fire with ſteel, and a yellowiſh grey ſubſtance, perfectly ſimilar

in

in appearance to that before deſcribed. In its vicinity the chert is extremely broken and cellular; and I am of opinion, that this is a continuation of the Gartneſs Whyn Dyke, it having the ſame range, and nearly the ſame breadth. After viewing the Fort (ſee PENNANT's Voyage in 1772, p. 219.), we began our examination of the ſubſtances which compoſe the hill. They are in general ſiliceous, variouſly blended; the northern brow is of chert, the joints of which range E. and W. and the beds are in ſome parts vertical, in others horizontal. Here is an irregular bed of iron ore, called emery by the inhabitants; it is from ſix to twelve inches in thickneſs, and may be traced about four fathoms in length. Above it is a ſtrange mixture of chert, hornſtone, and friable yellow ſand-ſtone, blended together without any order or regularity. This bed of iron ore is viſible on the S.W. part of the hill, where it is near two feet thick, is ſtratified with the chert, which lies in beds from three inches to two feet in thickneſs, and is covered with a cryſtalline ſand. On the ſouth ſide of the hill, where a trial has formerly been made for iron ore, there are ſeveral lumps collected in an heap, but no appearance of bed or vein. Where the rock has been laid bare, it is an hard chert, with an outer coat of ſoft yellow ſand-ſtone. Higher up the hill is an hard chert, with a kind of bladder-holes. Skirting the hill we deſcended on the north ſide, and ſaw two places, formerly wrought, where there is not any appearance of a vein, but merely a jumble of iron pyrites. At a very ſmall diſtance from hence, the limeſtone ſhews itſelf, and trials have been made for lead ore. Having ſurrounded the hill, and examined it attentively, and having ſeen the chert, of which it is chiefly compoſed, thrown into an infinite variety of forms; in ſome parts broken and indented, with a kind of empty bladder-holes;

holes; whilſt in other parts they are filled with a cryſtalline chert, with which the tops of the neighbouring mountains, and the Paps of Jura, abound: in ſhort, from the very rude and irregular appearance of the ſummit of the hill, from its riſing ſo ſuddenly from the limeſtone ſtrata, and from the Whyn Dyke which runs through it, and which may be traced ranging far away to the north-eaſtward, I am ſtrongly induced to believe it of volcanic origin.

If it be admitted that I am right in my opinion of the volcanic origin of theſe different ſubſtances, a large tract will then be added to that already proved by others to have been ſubject to the effects produced by ſubterraneous fire; which, as far as has hitherto been diſcovered with us, commences in the S.W. part of Derbyſhire, and, if I miſtake not, is again ſeen in Seathwaite, about five miles from Hawkſhead, in the N.W. part of Lancaſhire, and appears (N.W. from thence) in the neighbourhood of Belfaſt in Ireland, and ranging through the northern part of that kingdom; it is perceived in ſeveral of the weſtern iſlands of Scotland, extending as far north as the iſland of Lewis, which is the northernmoſt of the Hebrides, and croſſing eaſt from Ilay (which is the ſouthernmoſt) by Tarbut, Dumbarton, Stirling, and Edinburgh to Dunbar.

Some perſons may conſider, with aſtoniſhment, the extent of thoſe veins and maſſes of lava which appear in the northern part of the Britiſh iſles, where no crater is viſible; whilſt others, who have read VON TROIL, and recollect that he ſays (at p. 234.), "That lava is ſeldom found near the opening of "a volcano, but rather tuff, or looſe aſhes and grit," may perhaps unite with me in opinion, with Mr. WHITEHURST, "that the crater from whence that melted matter flowed, to-"gether with an immenſe tract of land towards the north,

"have

"have been abſolutely ſunk and ſwallowed into the earth, at "ſome remote period of time, and became the bottom of the "Atlantic Ocean. A period indeed much beyond the reach of "any hiſtorical monument, or even of tradition itſelf."

That you may more readily compare the ſpecific gravities of the Ilay lavas, and other ſubſtances (mentioned in theſe letters) with thoſe from other parts, I have annexed a table of their ſeveral weights; and remain, &c.

A. MILLS.

Ardlun Coal, - - - - -	1,284
Jet, according to Dr. WATSON, - -	1,236
	1,180
Cannel coal, from Haig in Lancaſhire, - -	1,275
Whyn Dyke, from near M'Arthur's head, N° 1.	2,863
Whyn Dyke from Freeport, inſide N° 2. -	2,881
outſide 3. -	2,850
Whyn Dyke from Gartneſs, N° 4. - -	2,631
5. - -	2,698
6. - -	2,484
7. - -	2,542
8. - -	2,322
9. - -	2,833
10. - -	2,652
Baſaltes from the Giant's Cauſeway, - -	2,743
——— from Fairhead, - - -	2,950
——— from Ardlun, - - - -	2,724
——— from Staffa, - - - -	2,736

Vitreſcent ſubſtance from Ardlun, - - 2,800
Toadſtone, from great rocks Derbyſhire, yellow grey, 2,133
dark compact, 2,634
ditto cellular, 2,528
——— yellow grey from Bonſal, 2,219

[Specimens of all theſe ſubſtances, except the Jet and Cannel Coal, accompanied the Paper.]

EXPLANATION OF THE FIGURES, TAB. IV.

Fig. 1. View of the Glen near Ardlun Head in Mull.
2. View of the inſulated Rock at the termination of the Glen.
3. A view of the great Fiſſure, the Cave, and the ſuſpended Stones, in the Iſland of Mull. The fiſſure ranges N. and S., is about ten feet wide and forty yards deep: the ſides and the ſuſpended ſtones are granite.

Fig. 1.

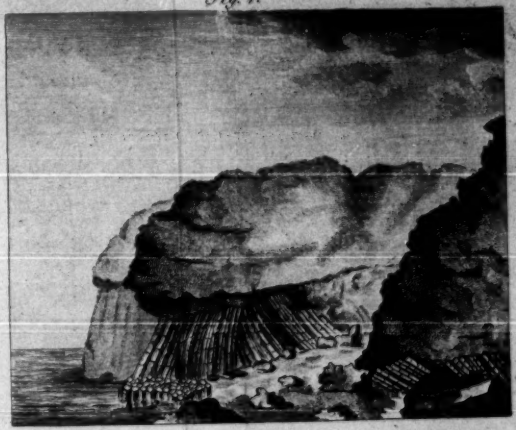
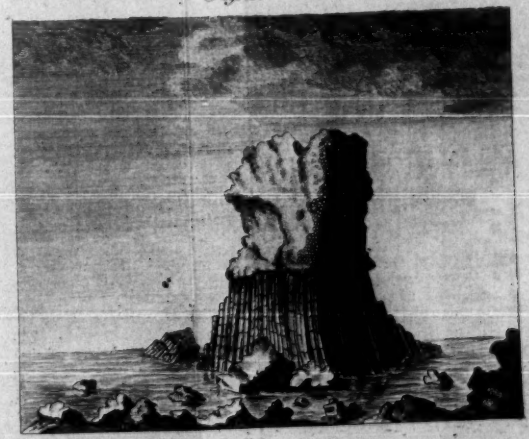


Fig. 3.

Fig. 2.



X. *On the Height of the luminous Arch which was ſeen on* Feb. 23, 1784. *By* Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read February 25, 1790.

THIS arch was obſerved, at the ſame time, at Cambridge by Mr. WOLLASTON; at Kimbolton in Huntingdonſhire, by the Rev. Mr. HUTCHINSON; and at Blockley near Campden in Glouceſterſhire, by Mr. FRANKLIN; and is deſcribed in letters from thoſe gentlemen read to the Royal Society in December 1786 *.

It has been remarked, that as the arches of the kind deſcribed in theſe Papers have uſually but a very ſlow motion, their height above the ſurface of the earth may readily be determined, provided they are obſerved about the ſame time, at places ſufficiently diſtant; and they ſeem to be the only meteors of the aurora kind whoſe height we have any means of aſcertaining.

The three places at which this phænomenon was ſeen are not ſo well ſuited for this purpoſe as might at firſt be expected from their diſtance, becauſe they lie too much in the direction of the arch; they however ſeem ſufficient to determine its height within certain limits, and perhaps are as well adapted for it as any obſervations we are likely to have of ſuch phænomena.

The latitude of Cambridge is 52° 12′ 36″: that of Kimbolton is ſaid by Mr. HUTCHINSON to be 52° 20′, and,

* See p. 43—46. of this Volume.

according to the ſurvey of Huntingdonſhire, publiſhed by JEFFERIES, is 52° 19′ 50″; ſo that we may ſuppoſe it to be ſeven geographical miles north of Cambridge, and by the maps it ſeems to be about 18 ſuch miles weſt of it: and Blockley is by the map 12 geographical miles ſouth and 72 weſt of Cambridge.

At Cambridge the obſervations of its track ſeem to have been made at about 9 h. 15′ P.M. or 8 h. ſidereal time. At Kimbolton, allowing for the difference of meridians, they could hardly have been made more than 5′ ſooner; and at Blockley they were moſt likely made nearly at the ſame time as at Cambridge.

At Blockley the arch paſſed about 7° ſouth of the zenith; but it is unneceſſary to determine this point with preciſion. At Kimbolton it was found by a quadrant to paſs 11° to the ſouth of it; and at Cambridge it was obſerved to paſs through δ and ε Tauri, β Aurigæ, θ Urſæ majoris, Cor Caroli, and Arcturus. Now, if an arch was drawn through theſe ſtars, it muſt, I think, have appeared ſenſibly waved to the eye; whereas Mr. WOLLASTON did not take notice of any crookedneſs in this part of its courſe. It is moſt likely, therefore, that the middle of the arch muſt have paſſed to the ſouth of β Aurigæ, and to the north of θ Urſæ; and if a circle is drawn through δ Tauri, Arcturus, and a point one degree north of the zenith, it will differ but little from a great circle, will agree as well with the poſitions of theſe ſtars as any regular line which can be drawn, and will paſs 2¼ degrees below β Aurigæ, and as much above θ Urſæ; which is not a greater difference from obſervation than may well have taken place, conſidering how much care and acquaintance with the fixed ſtars are required to determine a path by them ſo nearly.

The direction of the arch here described in that part near the zenith is W. 18° S.; and if a line is drawn through Cambridge in this direction, Kimbolton is 12,8 geographical miles north of it; and therefore, as the arch appeared 12° more south at Kimbolton than at Cambridge, the height of the arch above the surface of the earth must be 61¼ geographical or 71 statute miles. If we suppose that the middle of the arch really passed through β Aurigæ, the height comes out 52 statute miles. On the whole, I should think, the height could hardly be less than 52 miles, and is not likely to have much exceeded 71.

The common aurora borealis has been supposed, with great reason, to consist of parallel streams of light shooting upwards, which, by the laws of perspective, appear to converge towards a point; and when any of these streams are over our heads, they appear actually to come to a point, and form a corona. Hence, from analogy, it seems not unlikely, that these luminous arches may consist of parallel streams of light, disposed so as to form a long thin band, pretty broad in its upright direction, and stretched out horizontally to a great length one way, but thin in the opposite direction. If this is the case, they will appear narrow and well defined to an observer placed in the plane of the band; but to one placed at a little distance from it, they will appear broader, fainter, and less well defined; and when the observer is removed to a great distance from the plane, they will vanish, or appear only as an obscure ill-defined light in the sky.

There are two circumstances which rather confirm this conjecture: first, that though we have an account of another arch besides this * having been seen at great distances in the

* That of Feb. 15, 1750. Phil. Trans. XLVI. p. 472. and 647.

direction

direction of the arch, we have none of any having been ſeen in places much diſtant from each other in the contrary direction; and, ſecondly, that moſt of them have paſſed near the zenith, whereas otherwiſe they ought frequently to appear in other ſituations; for if they appeared near the zenith to an obſerver in one latitude, they ſhould appear in a very different ſituation in a latitude much different from that.

I wiſh it to be underſtood, however, that I do not offer this as a theory of which I am convinced; but only as an hypotheſis which has ſome probability in it, in hopes that by encouraging people to attend to theſe arches, it may in time appear whether it is true or not. If it ſhould hereafter be found, that theſe arches are never ſeen at places much diſtant from each other in a direction perpendicular to the arch, it would amount almoſt to a proof of the truth of the hypotheſis; but if they ever are ſeen at the ſame time at ſuch places, it would ſhew that the hypotheſis is not true.

Suppoſing the hypotheſis to be well founded, the height above determined will anſwer to the middle part of the band, provided the breadth of it was ſmall in reſpect of its diſtance from the earth, but otherwiſe will be conſiderably below the middle. If the breadth of the band was equal to the diſtance of its lower edge from the earth, the height of the lower edge would be three-fourths of that above found; and if the breadth was many times greater, would be half of it.

In the common aurora borealis, an arch is frequently ſeen low down in the northern part of the ſky, forming part of a ſmall circle. What this is owing to, I cannot pretend to ſay; but it is likely that it proceeds from ſtreams of light which appear more condenſed when ſeen in that direction than in any other, and conſequently that the ſtreams which form the arch to an obſerver

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in one place are different from those which form it to one at a distant place, and consequently that no conclusion as to its height can be drawn from observations of it in different places. Attempts, however, have been made to determine the height of the aurora from such observations, and even from those of the Corona*; though the latter method must surely be perfectly fallacious, and most likely the former is so too.

* Bergman. Opusc. Vol. V.

XI. *Obſervations on Reſpiration.*

By the Rev. Joſeph Prieſtley, *LL.D. F. R. S.*

Read February 25, 1790.

WHEN I wrote the Obſervations on the Subject of *Reſpiration*, publiſhed in the Philoſophical Tranſactions, Vol. LXVI. p. 226. I ſuppoſed, that in this animal proceſs there was ſimply an emiſſion of phlogiſton from the lungs. But the reſult of my late experiments on the mutual tranſmiſſion of dephlogiſticated air and of inflammable and nitrous air, through a moiſt bladder interpoſed between them, and likewiſe the opinions and obſervations of others, ſoon convinced me, that, beſides the emiſſion of phlogiſton from the blood, dephlogiſticated air, or the acidifying principle of it, is at the ſame time received into the blood. Still, however, there remained a doubt how much of the dephlogiſticated air which we inhale enters the blood, becauſe part of it is employed in forming the *fixed* air, which is the produce of reſpiration, by its uniting with the phlogiſton diſcharged from the blood: for ſuch I take it for granted is the origin of that fixed air, ſince it is formed by the combination of the ſame principles in other, but exactly ſimilar, circumſtances.

Dr. Goodwyn's very ingenious obſervations prove, that dephlogiſticated air is *conſumed*, as he properly terms it, in reſpiration; but, for any thing that he has noted, it may be wholly employed

employed in forming the fixed air above mentioned. He has proved, indeed, that the application of dephlogiſticated air to the outſide of a vein will change the colour of the blood contained in it. But this might have been effected, as I firſt ſuppoſed, by the ſimple diſcharge of phlogiſton from the blood, when it had an opportunity of uniting with the dephlogiſticated air thus preſented to it. He does not, however, ſeem to ſuppoſe, that there is any phlogiſton diſcharged from the blood in the act of reſpiration, but only that dephlogiſticated air enters into it. But that my former ſuppoſition, as well as *his*, is true, will appear, I preſume, from the experiments which I ſhall preſently recite.

As, in order to determine what proportion of the dephlogiſticated air deſtroyed by reſpiration is employed in forming the fixed air which is the produce of it, it was neceſſary to aſcertain as exactly as poſſible the proportion of dephlogiſticated air and of phlogiſton in the compoſition of fixed air, I repeated with particular care experiments ſimilar to thoſe which I had formerly made for that purpoſe.

I heated charcoal of copper in 41 ounce meaſures of dephlogiſticated air of the ſtandard of 0.33, till it was reduced by waſhing in water to 8 oz. m. of the ſtandard of 1.33. Again, I heated charcoal of copper in 40.5 oz. m. of dephlogiſticated air of the ſtandard of 0.34 till it was reduced to 6 oz. m. of the ſtandard of 1.76. And in each of theſe caſes there was a loſs of 6 gr. of the charcoal of copper; ſo that there cannot be more than 6 gr. of phlogiſton in 33 oz. m. of fixed air, and conſequently that only a very little more than one-fourth of the weight of fixed air is phlogiſton.

I heated perfectly well burned charcoal of wood in 60 oz. m. of common air, and found one-fifth of the remainder to

be fixed air, and the reſiduum of the ſtandard of 1.7. Laſtly, I heated 8¼ gr. of perfect charcoal in 70 oz. m. of dephlogiſticated air, of the ſtandard of 0.46, when it ſtill continued 70 oz. m.; but after waſhing in water it was reduced to 40 oz. m. of the ſtandard of 0.6, and the charcoal then weighed 1¼ gr.; ſo that from this experiment with common charcoal, as well as from the preceding with charcoal of copper, it appears, that about one-fourth of the weight of fixed air is phlogiſton, and conſequently that the other three-fourths are dephlogiſticated air.

Having done this, I proceeded to aſcertain how much fixed air was actually formed by breathing a given quantity both of atmoſpherical and of dephlogiſticated air, in order to determine whether any part of it remained to enter the blood, after forming this fixed air.

For this purpoſe I breathed in 100 oz. m. of atmoſpherical air, of the ſtandard of 1.02, till it was reduced to 71 oz. m. and by waſhing in water to 65 oz. m. of the ſtandard of 1.45. When the computations are properly made, as directed in a former Paper, it will appear, that, before the proceſs, this air contained 67.4 oz. m. of phlogiſticated air, and 32.6 oz. m. of dephlogiſticated air; that after the proceſs there remained 53.105 oz. m. of phlogiſticated air, and 11.895 oz. m. of dephlogiſticated air; and that there were only 6 oz. m. of fixed air produced; for the quantity abſorbed during the proceſs could only have been very inconſiderable. It will therefore be evident, that, in this experiment, 20.7 oz. m. of dephlogiſticated air, which would weigh 12.42 gr. diſappeared; whereas all the fixed air that was found would only have weighed 4.4 gr., and one-fourth of this being phlogiſton, the dephlogiſticated air that entered into it would have weighed only 3.3 gr.; conſequently

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9.12 gr. of it muſt have entered the blood, which is three times as much as that which did not enter, but was employed in forming the fixed air in the lungs.

I breathed in 100 oz. m. of dephlogiſticated air, of the ſtandard of 1.0, till it was reduced to 58 oz. m., and by waſhing in water to 52 oz. m. of the ſtandard of 1.75, with two equal quantities of nitrous air. The computations being made as before, it will appear, that, before this proceſs, this air contained 66 oz. m. of phlogiſticated, and 34 oz. m. of dephlogiſticated air; and that after the proceſs there were 30.368 oz. m. of phlogiſticated air, and 21.632 oz. m. of dephlogiſticated air. In this caſe, therefore, the dephlogiſticated air that diſappeared was 13.3 oz. m. weighing 7.8 gr. and the fixed air was 6 oz. m. weighing 4.4 gr.; ſo that here alſo about three times as much entered the blood as did not.

Theſe experiments I repeated many times, and though not with the ſame, yet always with ſimilar, reſults, the greateſt part of the dephlogiſticated air, but never the whole, paſſing the membrane of the lungs, and entering the blood.

When the reſults above mentioned are compared, it will appear, though the obſervation eſcaped Dr. Goodwyn, that part of the phlogiſticated air entered the blood, as well as the dephlogiſticated air; or, which is the ſame thing, that the dephlogiſticated air which was conſumed was not of the pureſt kind. This experiment I repeated ſo often, and always with the ſame reſult, that I am confident I cannot be miſtaken in this concluſion. This fact, of which I had no previous expectation, I firſt thought might be accounted for by ſuppoſing, that the two conſtituent parts of atmoſpherical air, *viz.* the phlogiſticated and dephlogiſticated air, are not ſo *looſely mixed* as has been imagined; but rather that they have ſome principle

of *union*, ſo that, though they may be completely ſeparated by ſome chemical proceſſes, they are not entirely ſo in this; but that the dephlogiſticated air, paſſing the membrane of the lungs, carries along with it ſome part of the phlogiſticated with which it was previouſly combined. But, at the obliging ſuggeſtion of Dr. BLAGDEN, I now think it more probable, that the deficiency of phlogiſticated air was owing to the greater proportion of it in the lungs *after* the proceſs than *before*.

There will always be ſome uncertainty in the reſults of the long-continued reſpiration of any kind of air, as at the laſt the operation becomes laborious, and the quantity inſpired and expired is therefore much greater than at the firſt. But I was aware of this circumſtance, and endeavoured to obviate the effects of it, by leaving off with my lungs as nearly as I could judge in the ſame ſtate of diſtention as when I began, which was always after a moderate expiration; ſo that two or three ounce meaſures would have made a very ſenſible difference, as any perſon will find by actual trial.

XII. *An Account of the Trigonometrical Operation, whereby the Diſtance between the Meridians of the Royal Obſervatories of* Greenwich *and* Paris *has been determined. By Major-general* William Roy, *F. R. S. and A. S.*

Read February 25, 1790.

INTRODUCTION.

THE trigonometrical operation which becomes the ſubject of the preſent Paper, had its commencement, as will be remembered, in the meaſurement of a baſe on Hounſlow-Heath in 1784, an account of which was given to the Royal Society in the following year.

On the completion of that firſt part of the buſineſs, it was little expected, that nearly three full years would have elapſed before, even in this country, an inſtrument could be obtained for taking the angles!

In the ſpring of 1787, there were indeed appearances, that Mr. RAMSDEN would have enabled us to embrace the early part of the ſeaſon, by proceeding with the execution of the main deſign; and therefore Sir JOSEPH BANKS had opened (through the official intercourſe of his Majeſty's Secretary of State, the Marquis of CARMARTHEN, with the Ambaſſador at the Court of France) a correſpondence with the Academy of Sciences, regarding the co-operation expected on their part, for connecting the triangles which we were now preparing to

extend along the Engliſh coaſt, with thoſe formerly executed on the coaſt of France, oppoſite to Dover. And Dr. BLAGDEN (who for this purpoſe conſented to lay aſide his intention of making a tour through Germany that ſummer) had engaged to aſſiſt in the buſineſs, on the appointment of the Royal Society, whenever we ſhould be enabled to aſſign any probable time, for the different parties to repair to their reſpective coaſts, for the aforeſaid co-operation.

About the ſame time likewiſe, a Paper was laid before the Royal Society, intended as a ſketch of the mode propoſed to be followed in carrying the ſcheme into execution; for which purpoſe it was accompanied with a general map, ſhewing nearly the diſpoſition of the triangles, and containing alſo various inveſtigations concerning the figure of the earth, whereon, it is hoped, the reſult of the preſent operation will throw ſome additional light.

For ſeveral months of the ſpring and ſummer of 1787, Mr. RAMSDEN had been ſeriouſly at work in endeavouring to finiſh the inſtrument. Not having employed a ſufficient number of workmen upon it at the outſet, it was now evident, that he had even deceived himſelf, by leaving too much to be done at the latter end. At length, however, the inſtrument was produced, and placed on the 31ſt of July at the ſtation near Hampton Poor-houſe, on the very ſpot where, about thirty-five months before, the meaſurement of the baſe had been compleated.

By commencing an operation of this nature, at ſo advanced a ſeaſon of the year, it was ſufficiently obvious, that only very faint hopes could be entertained of bringing it to a concluſion before the bad weather would ſet in. But it being of much importance to get the triangles, which extend acroſs the

Channel, at all events executed, it was therefore propoſed to Comte DE CASSINI, who by this time had been appointed by the Academy of Sciences to ſuperintend their part of the buſineſs, that he ſhould fix the time that might ſuit him beſt for our meeting on the coaſt; that we would then diſcontinue the operation to the weſtward, and, having in concert executed the coaſt triangles, we would reſume the inland parts of our own ſeries at ſome more convenient opportunity.

This propoſition being readily acceded to by Comte DE CASSINI, the 20th of September was appointed for our repairing to the coaſts of Dover and Calais reſpectively.

In the mean time our operation was continued, with all imaginable care and aſſiduity, through the firſt ten ſtations of the ſeries of triangles from Hampton Poor-houſe to that at Wrotham Hill incluſively.

The inſtrument, and the various parts of the apparatus, were then removed to Dover, at which place Meſſ. DE CASSINI, MECHAIN, and LE GENDRE, three diſtinguiſhed Members of the Academy of Sciences, arrived on the 23d of September.

In the courſe of two days that theſe Gentlemen honoured us with their company at Dover (and we regretted exceedingly that the lateneſs of the ſeaſon did not admit of our enjoying that pleaſure for a much longer period) every thing was ſettled in the moſt amicable manner poſſible, with regard to the times of reciprocal obſervation.

A great number of white lights, fitted for long diſtances, and ſeveral reverberatory lamps had been previouſly provided. Having been ſupplied with ſuch a proportion of the lights as ſeemed neceſſary for their ſide of the channel, and one of the lamps, the French Gentlemen departed for Calais on the 25th, accompanied by Dr. BLAGDEN, who attended them during the

time of the co-operation, until it was finally closed on the 17th of October.

For the greater part of the time, the weather was extremely bad; nevertheless, on the particular nights when the most important observations on our side were made, namely, those at Dover and Fairlight Down, the nights happened very fortunately to be favourable, so as to enable us to intersect, with great accuracy, the two distant points on the French coast of *Blancnez* and *Montlambert* *, and thereby to establish for ever, the triangular connection between the two countries.

The Duke of RICHMOND, Master General of his Majesty's Ordnance, had, in the most liberal manner possible, given every assistance to the operation (from that great department over which he presides with so much honour to himself and advantage to the publick) by furnishing an officer and a detachment of artillery-men for the work; ordering the laboratory at Woolwich † to supply whatever fire-works might be wanted for signals; and temporary scaffolds to be erected at Greenwich Observatory, Shooter's Hill, and Dover Castle, for the reception of the instrument. But what was still of more importance than any of these, his Grace had permitted Lieut. FIDDES (one of the engineers on the survey then under my direction) to be employed, in the summers of 1786 and 1787, in making a very accurate plan of that part of Romney Marsh where the base of verification was to be measured. In a country so much intersected by ditches, and where there were so many ponds of water to be avoided, without such a plan raised before-hand,

* The name of this hill is vulgarly pronounced *Boulemberg*, and it is even written in the same manner in the book, *La Méridienne vérifiée*.

† Major Congreve, of the Royal Artillery, had the management of the lights at Shooter's Hill; and his assistance was found to be most essentially useful.

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an operation of ſo delicate and difficult a nature could not have been effected.

The apparatus for the meaſurement of the baſe with the ſteel chain, notwithſtanding the urgency of the caſe, was not ſent to its deſtination until the end of the firſt week of October. To Lieut. FIDDES the engineer, was then joined Lieut. BRYCE of the Royal Artillery; and it was not before the beginning of December, that theſe two gentlemen, with the moſt unremitting labour and perſeverance, were able to accompliſh the meaſurement, as will be ſeen in the detailed account of that operation given in the firſt ſection of this Paper.

In finiſhing the co-operation with the French Commiſſioners, at Lydd on the 17th of October, our inſtrument had now paſſed through ſixteen ſtations out of twenty-three. There of courſe remained yet ſeven ſtations where it was to be placed, and obſervations to be made. Eagerly wiſhing to bring the buſineſs to a concluſion, we ſtruggled on through five of the ſeven. But the weather at length became ſo tempeſtuous, that it was utterly impoſſible to continue it, with any hopes of being able to make ſatisfactory obſervations. Perched on the tops of high ſteeples, ſuch as Lydd and Tenterden, or on heights, ſuch as Hollingborn Hill, we ſufficiently experienced, that operations of this ſort, where the moſt important obſervations could only be made at night, by means of the white lights, ſhould never be undertaken in the latter ſeaſon.

On the ſecond of November, the inſtrument was accordingly removed from the top of Hollingborn Hill, and ſent to town, leaving the ſtations on Goudhurſt and Frant Churches, both likewiſe ſituated on eminences, unoccupied until the enſuing ſeaſon.

The winter months were employed in calculating the obſervations that had been made; and from theſe we were very well enabled to judge to what a degree of accuracy we had arrived in determining the ſides and angles: for Frant and Goudhurſt, having been interſected from Botley Hill, Wrotham Hill, and Hollingborn Hill; Goudhurſt having been obſerved from Tenterden, and Frant having (contrary to our expectation) been ſeen and obſerved from Fairlight Down, we had thereby the certain means of determining very nearly what difference there would be between the meaſured and computed length of either baſe as given by the other, although obſervations had not been made at the two intermediate ſtations of Goudhurſt and Frant. This difference, it was ſeen, would ſcarcely amount to one foot, or about $\frac{1}{2000}$th part of the whole diſtance. In as far, therefore, as the reſults of the plane triangles were concerned, we might have proceeded with the computations, and drawn the conſequent concluſions, without heſitation, or any riſk of ſenſible error.

But, beſides that it might ſtill have been ſaid that the inſtrument had not been placed at theſe two ſtations, there were reaſons of a different kind, which rendered it in ſome degree neceſſary to place the inſtrument not only at Goudhurſt and Frant, but alſo at Botley Hill and Folkſtone Turnpike, where it had formerly ſtood.

In 1787, when at the ſtation of St. Ann's Hill, in a very high wind, the box containing the axis level was blown from the ſcaffold, and unluckily broken. Mr. RAMSDEN replaced it with one not ſo good as the firſt; and it was with this ſecond level that the obſervations of the pole ſtar had been made at Dover Caſtle. This caſtle, although lofty, and ſituated on a high chalk cliff, that raiſes its northern turret about 466 feet

above low water at ſpring tides, is nevertheleſs ſurrounded on the land ſide with eminences, at the diſtance of ſix or ſeven miles, ſtill higher than itſelf. From this circumſtance we found it impoſſible to connect it with the great triangles to the weſtward, otherwiſe than by a ſhort ſide. It was therefore ſufficiently obvious, that it would be eligible to make obſervations of the pole ſtar for determining the difference of longitude, and the convergence of the meridians, at ſome other intermediate ſtations between Greenwich and Dover, from whence our longeſt ſides could be diſtinctly ſeen. For this purpoſe none ſeemed ſo proper as Botley Hill, Goudhurſt, and Folkſtone Turnpike. The firſt of theſe three is only 171¼ feet eaſtward from the meridian of Greenwich, Goudhurſt is about 23 miles ſouth-eaſtward from the former, and Folkſtone Turnpike, the ſtation neareſt to Dover, is ſo ſituated, that from it can be ſeen the end of the baſe of verification at High Nook, Fairlight Down, and other diſtant ſtations.

With this object in view, whereon ſo much depended, we had again the mortification to be thrown into the latter ſeaſon of 1788.

Beſides a better level for the axis of the teleſcope, the microſcope B wanted to be better ſupported. Another ſort of clamp, alſo an eye-piece, with a diagonal priſm for obſervations near the zenith, or for thoſe of the pole ſtar in high latitudes, were neceſſary improvements, which might have been executed in a ſhort ſpace of time. With theſe alterations the inſtrument was at laſt returned, but ſo late, that it could not be placed on Goudhurſt Steeple till the 9th of Auguſt, 1788.

The obſervations at Goudhurſt, Frant, Botley Hill, and Folkſtone Turnpike, having been finiſhed early in September, the

the inſtrument was brought back to town, in the neighbourhood of which it was employed for three days for the following purpoſe.

In 1787, when at the ſtations of Hundred Acres, Norwood, Greenwich, and Shooter's Hill, we had only been able to determine, in a ſatisfactory manner, two points within the limits of the Capital, namely, St. Paul's Church and Argyll Street, the laſt by means of the white lights. Bearings of ſome others, it is true, were obtained; but, in order that theſe might be interſected in the beſt manner, it became neceſſary to place the inſtrument at one or more ſtations to the northward of the town.

With the view, therefore, of laying the foundation hereafter for a much more accurate plan of London than could poſſibly be obtained in any other way, the inſtrument was placed, firſt, at Hornſey Hill, to the eaſtward of Highgate; and, ſecondly, on Primroſe Hill, between London and Hampſtead.

Although the weather was rather unfavourable at the time of making the obſervations from theſe two new ſtations; and that the ſmoke conſtantly hanging over the town in the latter ſeaſon impeded us greatly; nevertheleſs, the former bearings were interſected, and the ſituations of a conſiderable number of remarkable ſteeples within London and its environs, were accurately determined, as will more fully appear in treating of the ſecondary triangles.

Having thus briefly ſhewn the order with regard to time in which the recent operation, through its various ſteps, was progreſſively carried on and compleated, it is proper that I ſhould mention, that Mr. Dalby, who had been recommended as an aſſiſtant, has acquitted himſelf throughout the whole perfectly to my ſatisfaction, as a diligent and accurate

obſerver, as well as an able and indefatigable calculator. This teſtimony, which is juſtly due to his merit, joined to the ſpecimen which he gives of his mathematical abilities in the fifth ſection of this Paper, can ſcarcely fail of making him better known hereafter; and it is hoped, that he will have opportunities of exerting his talents, by aſſiſting in the continuation of the future operations that are projected and recommended to be carried on in the concluſion of this Memoir; the various parts of which are arranged as follows:

Section Firſt.

Deſcription of the apparatus made uſe of in the meaſurement of the baſe of verification in Romney Marſh, with the hundred-feet ſteel chain, in the autumn of 1787, with the reſult of that operation. Reference to be had to Plate I. and II., and alſo to the table containing the general detail of the meaſurement.

Section Second.

General deſcription of the great inſtrument with which the angles in the recent trigonometrical operation were obſerved; ſhewing alſo its various adjuſtments for practice. Reference to be had to Plate III. a general view of the entire machine; Plate IV. a plan and two ſections of it; Plate V. various parts repreſented on large ſcales; and Plate VI. the microſcopes and eye pieces.

Section Third.

Deſcription of various articles of machinery made uſe of in the courſe of the trigonometrical operation, referred to in Plate VII. Alſo the diſtinction of the ſtations into two ſets; thoſe of the ſecond ſet being referred to in Plate VIII.

Section

Section Fourth.

Calculation of the ſeries of triangles extending from Windſor to Dunkirk, whereby the geodetical diſtance between the meridians of the Royal Obſervatories of Greenwich and Paris is determined. Reference to be had to Plate IX.

Section Fifth.

On the difference between horizontal angles on a ſphere and ſpheroid. Plate X.

Section Sixth.

Manner of determining the latitudes of the ſtations. Application of the pole ſtar obſervations to computations on different ſpheres, and alſo on M. BOUGUER's ſpheroid, for the determination of the difference of longitude. Ultimate reſult of the trigonometrical operation, whereby the difference of the meridians of the Royal Obſervatories of Greenwich and Paris is determined. Plate X.

Section Seventh.

An account of the obſervations made during the courſe of the trigonometrical operation for the determination of terreſtrial refraction. Plate X.

Section Eighth.

Secondary triangles, ſubdivided into two ſets, for the improvement of the maps of the country, and the plan of the City of London and its environs. Plate XI.

Concluſion,

Containing Propoſitions for extending trigonometrical operations over Great Britain.

SEC-

SECTION FIRST.

Deſcription of the apparatus made uſe of in the meaſurement of the baſe of verification in Romney Marſh, with the hundred-feet ſteel chain, in the autumn of 1787, *with the reſult of that operation. Reference to be had to Plates I. and II.; and alſo to the table containing the general detail of the meaſurement.*

Article I. *Preamble.*

IN the account of the meaſurement of the baſe on Hounſlow Heath in 1784, which appeared in the Philoſophical Tranſactions of the ſubſequent year, we had occaſion to ſhew, how very accurately diſtances might be determined by the ſteel chain, when applied in the ordinary way on the natural ſurface of the ground, if that ſurface happened to be tolerably ſmooth, which was the caſe in the inſtance alluded to. By the compariſon of the meaſurement of a length of one thouſand feet with the glaſs rods, and with the chain when uſed with an apparatus adapted to the purpoſe, it further appeared, that the difference between the reſults was ſo very ſmall as ſcarcely to be diſcernible, ſince it would not have exceeded half an inch on the whole length of that baſe of 27404.7 feet.

Having always conſidered the experiment on Hounſlow Heath, juſt now mentioned, as poſitive proof of the excellency of the chain, it had been reſolved on to apply it to the menſuration of the baſe of verification in Romney Marſh, even if no other reaſons had exiſted to make that choice eligible.

But beſides the danger of having the glaſs rods broken, in tranſporting to ſo great a diſtance from London, and, on ſuch an event happening, the impoſſibility of getting them replaced with others at the advanced ſeaſon of the year in which we were unfortunately thrown with the operation, it was obvious, that in a plain of the breadth of ſix miles, ſo much interſected with ditches full of water as Romney Marſh in reality is, the laying of bridges for the tripod ſtands, which muſt have been uſed with the glaſs rods, would alone have been a very troubleſome and tedious operation.

ART. II. *Beech Poſts.*

In the firſt place, about thirty poſts made of beech wood, three inches in diameter, and of different lengths, from two feet three inches to three feet ſix, and a few of them ſtill longer, were provided. They were ſhod with iron, and each of them carried on its top a caſt-iron ferrule, with two dovetails projecting from it; care being taken in driving them into the ground, that the dovetails ſhould ſtand in or nearly in the direction of the baſe, as repreſented by the plan and ſection of a ſingle poſt, in the middle part of Plate I. The arrangement of twenty-four of theſe poſts may be ſeen at the top of the ſaid plate, for the meaſurement of a portion of the baſe equal to one hundred yards, or the length of three chains. Sixteen of the poſts reckoning from that which ſtands in the center of the firſt group, to that which ſtands in the center of the ſecond, and ſo on from right to left, were placed at the diſtance of twenty feet from each other. The firſt is ſuppoſed to co-incide with the mouth of the pipe ſunk into the earth, at the eaſtern extremity of the baſe, at a place called High Nook near Dymchurch; and every fifth poſt from that towards the

left

left marks the end of a chain. The other eight poſts in the arrangement, that is to ſay, the right and left poſts of each of the four groups, are ſuppoſed to ſtand twelve or fifteen inches from thoſe in the center. By referring to the elevation near the top, and the plans and ſection in the middle part of Plate I. it will be perceived, that theſe poſts, together with certain other iron parts of the apparatus fixed to them, hereafter to be deſcribed, ſupport the ends of the coffering for each chain, free and independently of the central poſts, to which laſt the braſs ſcales alone are attached.

ART. III. *Deal Coffers.*

Fifteen deal coffers, numbered from one to fifteen, were neceſſary for the length of three chains, being five to each. Six of them, that is to ſay, the firſt and fifth, the ſixth and tenth, the eleventh and fifteenth, being the firſt and laſt of each chain, were only nineteen feet four or five inches in length. The other nine, being the three in the middle of each chain, were of the complete length of twenty feet. Theſe coffers perfectly reſembled in ſhape, and nearly in dimenſions, the caſes of the glaſs rods, being ten inches broad in the middle, and uniformly of that depth throughout their whole length. But from the middle they became gradually narrower, in a curvilinear manner, towards each end, where they were only two inches wide. The two cheeks or ſides were about half an inch thick, and the bottom, which entered into a ſhallow groove in the middle of the cheeks, was an inch in thickneſs. Thus the cheeks being thin, bent and applied eaſily to the bottom, to which they were firmly nailed, and the whole was fortified by ſmall blocks of wood faſtened at intervals in the inſide, ſometimes above and ſometimes below the bottom.

From the elevation it will be perceived, that nine or ten inches of the under extremities of the cheeks were cut off, ſo as to permit the bottom itſelf to reſt on the irons. This conſtruction of the coffers was found to anſwer very well, that is to ſay, they were, conſidering their length, not ſo heavy as to be unmanageable, at the ſame time that by their general figure, and particularly the depth of the cheeks, they were entirely prevented from warping.

In addition to the fifteen coffers, juſt now deſcribed, a ſixteenth, not repreſented in the plate, was afterwards prepared at Hythe by Lieut. Findes, to be uſed occaſionally, when the end of one chain, and commencement of another, co-incided with a deep ditch or one of the ſewers full of water, and where of courſe it would have been extremely difficult, if not impoſſible, to have fixed ſteadily the group of three poſts in the uſual manner. In this coffer there was a double or falſe bottom, with grooves adapted for the purpoſe; and the braſs ſcale, pulley, &c. were removed from the irons, and placed on this bottom.

Art. IV. *Apparatus of caſt iron, &c. for the ends of the Chain.*

By referring to the plate, where the ſeveral parts of the apparatus for the extremities of the chain are repreſented in plan and ſection, by a ſcale equal to one-fourth of their real dimenſions, it will appear, that the caſt-iron pieces were of two different forms, one long, and the other ſhort; but both applied in the ſame manner, on the ferrules binding the tops of the poſts, as has been already mentioned. Of the long kind there were in all fifteen or ſixteen, that is to ſay, one for each poſt in a length of three chains. Each iron had two clamps on its

under ſide, which being ſlackened, it was placed on its ferrule at right angles to the line of meaſurement; and being turned round 90°, the dovetails of the ferrule, ſtanding originally in the direction of the baſe, came within the clamps, which were then tightened by four ſcrews, turned with ſquare keys adapted to the purpoſe.

It is ſufficiently obvious, that ſo many irons, with ſuch a number of ſcrews to each, could not fail of rendering this operation tedious! The buſineſs would have been greatly expedited if there had been only two ſuch ſcrews, one on each end, in a middle ſituation; and, inſtead of the four ſcrews, there ſhould have been four ſteady pins, entering eaſily into holes prepared for them in the under ſide. A ſhort groove, of two or three inches in length, in each extremity of the bottom, would, on this ſuppoſition, have been neceſſary to ſuffer the ſquare heads of the ſcrews to paſs; and it will be readily conceived, that the thickneſs of the bottom would have effectually ſecured the chain from touching them, prevented the mutilation of its handles, and ſaved much loſs of time. Indeed the ſame purpoſe might have been effected, but not ſo advantageouſly, by laying the original four ſcrews lower in the iron, which its thickneſs eaſily admitted of. Finally, in order to avoid ſuch like inconveniencies in future, there is ſtill one imperfection more, which it is incumbent on me to remark, namely, that caſt-iron ferrules will not anſwer; for the force that was found to be neceſſary to drive the poſts into the ground, burſt almoſt the whole of them, ſo that before the operation was compleated, they were obliged to be replaced with others made of hammered iron, forged for the purpoſe.

Of the ſhort irons only three were neceſſary, one for each end of the chain, and a ſpare one in caſe of accident. They were

were placed, turned, and clamped on the ferrules, in all reſpects ſimilarly with thoſe of the long kind. By inſpection of the plate it will be ſeen, that each of them carried on its ſurface a braſs ſcale of ſix inches in length, divided into inches and quarters, and moveable in a ſlide, either backwards or forwards, by a finger-ſcrew adapted to the right-hand end.

The right-hand poſt of each group is called the *drawing-poſt*, becauſe the iron fixed on its top carries a ſmall apparatus of braſs, which being connected with the flat iron rod and hooks formerly uſed at Hounſlow Heath, for a like purpoſe, lays hold of the rear handle of the chain, and draws it back until zero co-incides with the point of commencement. The left-hand poſt in each group is called the *weight-poſt*, becauſe it carried a braſs pulley, over which a weight of 28 lbs. was hung by a ſmall rope attached to the hooks that laid hold of the front handle of the chain. This weight acting againſt the force of the ſcrew at the other end, the chain was thereby kept perfectly ſtraight in the coffers, and conſtantly in the ſame degree of tenſion, until ſome certain diviſion (the neareſt for inſtance) of the ſcale could be brought, by means of the ſcrew, accurately to coincide with 100 feet at the front end. That diviſion, whatever it might be, was of courſe regiſtered in the field book of the operation, together with the true temperature of the chain, as ſhewn by five thermometers, one being laid for that purpoſe in each coffer, and ſecured with white cloth from the ſun's rays, as occaſion might require.

Fifteen coffers were always arranged on the ground at the ſame time, comprehending a ſpace of the baſe equal to the length of three chains or 100 yards. The extremities of the firſt chain having been accurately transferred, in the manner above mentioned, to the braſs ſcales on the tops of the central

poſts, and theſe remaining firm and motionleſs, as being wholly unconnected with any other parts of the apparatus, the chain was then moved forward into the ſecond ſet of coffers, where the thermometers were alſo placed. In the mean time, the firſt ſet of coffers now vacated, with their poſts, &c. were carried on and arranged in the front, for the meaſurement of the ſecond 100 feet; and ſo on continually with the others in ſucceſſion.

ART. V. *Of the ſurvey of Romney Marſh previouſly to the meaſurement of the Baſe.*

In the introduction to this Paper it has been mentioned, that the Duke of RICHMOND had permitted Lieut. FIDDES, of the Royal Engineers, to be employed in 1786 and 1787 in raiſing a plan of that part of the Marſh where, on examination, it ſhould be found, that the baſe of verification might be the beſt executed. In juſtice to that officer, I conſider it as incumbent on me to ſay, that it was impoſſible for any perſon to fulfil the duties entruſted to him better than he did, either in the courſe of the ſurvey, or ſubſequent meaſurement of the baſe, whereof he alſo had the direction. The general inſtructions given to him were, that after having by a baſe of his own determined certain triangles in the neighbourhood of Dymchurch, Ruckinge, and Romney, by way of foundation for his work, he ſhould preſerve Ruckinge as the point whereon the *allignement* of the great baſe was to be directed, and vary the poſition of that end next the ſea-wall in ſuch a manner as to meet with the feweſt local obſtructions to the meaſurement between the two extremities. By inſpection of the plan Plate II. which comprehends a tract of country of two miles in breadth, one on each ſide of the baſe line, it will be perceived, that

that beſides the numberleſs ditches with which this ſingular plain is interſected, and which it was impoſſible to avoid croſſing, there is almoſt in every field a watering pond for the cattle, many of them of conſiderable depth. Nevertheleſs, ſo very attentive had Mr. FIDDES been to the accuracy of his ſurvey, that he was enabled, after ſeveral trials of other directions, to run a line from High Nook on Dymchurch Sea-wall, upon the ſmall ſpire of Ruckinge Church, of the length of nearly ſix miles, without interfering with any one of the watering ponds, or meeting with any other local obſtruction of conſequence. So very minute was he in his remarks, and ſo accurate in the ſituation of particular trees, that in tracing his line with the teleſcope, he managed ſo as to avoid them all, a few inſignificant buſhes excepted; which I believe to be an inſtance of exactneſs ſcarcely to be equalled.

ART. VI. *Pipes ſunk in the ground.*

Permiſſion having previouſly been obtained from the proprietors of the ſoil, pipes were ſunk into the ground at the two extremities of the baſe, and alſo one on Allington Knoll, which laſt point with Lydd Church * form that ſide of one of the great triangles depending on the baſe on Hounſlow

* It will be perceived, that ſeveral of the names of places differ in their orthography, from that whereby they were expreſſed in the plan of the intended triangles given in the Paper of 1787. This has been done, on procuring better information in that reſpect than had formerly been obtained. Mr. COLE, of Lydd, an ingenious gentleman, well acquainted with Romney Marſh, was ſo obliging as to preſent me with a manuſcript map of that ſingular plain, compiled by himſelf from actual ſurveys, where the names and boundaries of the *waterings*, and many other curious particulars, are very diſtinctly expreſſed.—Our plan of the baſe has therefore derived advantage by adhering to ſuch reſpectable authority.

Heath,

Heath, to be firſt verified by the meaſurement of this new baſe. Every field is ſurrounded with a ditch, in cleaning of which the earth and mud are continually thrown out on each ſide, whereby flat dykes are gradually formed on either ſide. That the meaſurement might be carried on as nearly as poſſible in the ſame plane, that is to ſay, about fifteen or eighteen inches above the common ſurface, therefore, narrow grooves were cut in theſe flat dykes, which the different farmers readily conſented to without murmuring. Here it is to be obſerved, that there was no occaſion for levelling the line, Romney Marſh having been formerly covered by the ſea, and a conſiderable part of it, particularly towards the bottom of the range of hills that ſeparate it from the Wealds of Kent, being ſtill lower than the ſea at high water, would again be overflowed by it, if much care and expence were not annually beſtowed in ſecuring and repairing the dykes, whereby it is protected. Thus the line of the baſe may be conſidered as an inclined plane, deſcending gradually about five feet from the mouth of High Nook pipe to within 246 yards of the Ruckinge end, where the ground in that direction ſeems to be the loweſt. Thence it riſes comparatively ſuddenly, about fifteen feet, to the mouth of the pipe ſituated in a ſmall field immediately adjoining to Ruckinge Church-yard.

ART. VII. *Reſult of the meaſurement.*

Lieut. FIDDES, in the courſe of his trigonometrical ſurvey, and of the different meaſurements he had actually made of the line with a common iron chain, which from time to time was compared with ſtandard rods of deal, had determined the total length of the baſe within a few feet of the truth, before the ultimate operation began. He had likewiſe driven into the ground, at the end of every

thouſand feet, a ſtrong picket, which were numbered 1, 2, 3, &c. from the pipe at High Nook to the 28th near Ruckinge. In all this preparatory part of the buſineſs he had no other aſſiſtants than the artillery-men of his ſurveying party. But for the ultimate determination, it being abſolutely neceſſary that he ſhould have the aid of ſome perſon in whom he could confide for the management of the operation in general, and particularly for the adjuſtment of the ſcale at one end of the chain, while he himſelf was adjuſting that at the other; therefore Lieut. BRYCE, of the Royal Artillery (now of the Corps of Royal Engineers), an attentive officer and excellent mathematician, was left with him for thoſe eſſential purpoſes. Theſe two gentlemen began the operation on the 15th of October, and, after experiencing many difficulties ariſing from the badneſs of the weather in that late ſeaſon of the year, and the defectiveneſs of the apparatus, it was only by dint of great labour, and the utmoſt perſeverance, that they were enabled to accompliſh the meaſurement on the 4th of December following.

The annexed general table of the baſe, which contains five columns, ſhews the progreſs that was made in the work from day to day. The firſt column contains the date; the ſecond, the ſpaces meaſured each day, reckoned by hundreds of yards, and denoted in the general plan by ſtrong dots; the third ſhews the temperature of the chain deduced from the mean of fifteen thermometers, five for each chain; the fourth expreſſes the difference of temperature above or below 62° of FAHRENHEIT; and the fifth ſhews the correction anſwering to that difference, additive to the apparent length with the ſign +, and ſubtractive from it with the ſign −.

	Feet.	In.	Pts.
From inſpection of the table it will appear, that the total apparent length of the baſe, as given directly by the ſteel chain, was $9512\frac{3474}{10000}$ yards; which are equal to	28536.	8.	835
But when the new points, at the diſtance of twenty-five feet from each other, were laid off on the chain in Mr. RAMSDEN's ſhop from the original points on the great plank of New-England deal, the temperature was 55°, that is, 7° below 62°; wherefore the contraction of the chain by 1° of FAHRENHEIT being = 0.00763 in. this × 7° × 285.37 chains = 15.242 in. is the reduction for the total contraction below 62°, to be taken from the apparent length; which are equal to - - - -	1.	3.	242
The apparent length is likewiſe to be leſſened by the exceſs of the corrections with the ſign − above thoſe with the ſign + in the annexed table; becauſe the temperature of the chain, when actually applied to the meaſurement, being ſo much below 62°, the apparent length became thereby too great by 30.65 inches, which are equal to	2.	6.	65
To be deducted alſo from the apparent length, the reduction on two hypothenuſal diſtances, meaſured at the Ruckinge extremity of the baſe, which is ſuddenly elevated above the loweſt part fifteen feet, amounting to - -	0.	3.	023
The ſum of theſe three reductions, to be taken from the apparent length, amounts to -	4.	0.	915

	Feet.	In.	Pts.
And consequently there remains for the length - - - -	28532	7	92

But when the chain was adjusted in Mr. RAMSDEN's shop, as above-mentioned, the temperature was 55°. Being then carried into St. James's Church-yard, its length was laid off on brass pins inserted into the stone coping of the church-yard wall, for the purpose of comparison on any future occasion, at which time the temperature had changed to 55°¼. After the measurement in Romney Marsh had been finished, the chain in the temperature of 39°, being stretched out on the wall, its length was found to fall short of the original points on the brass pins $\frac{103}{1000}$ of an inch. Now, 55°.5 − 39° = 16°.5, and 16°.5 × 0.00763 = 0.126 in.; hence 0.126 − $\frac{103}{1000}$ = 0.023 in. is the space which the chain had only lengthened during an operation which continued above six weeks; and one-half of this space, *viz.* 0.0115 multiplied by 285.37 chains is = 3.282 in., the correction to be added to the apparent length for the wear of the chain during the operation - - - +0. 3.282

Whence the length becomes -	28532	11	202

Lastly, the base is to be shortened for its height of 15¼ feet above the mean level of the sea, supposed to be 6 feet 8 inches above low-water spring tides at High Nook, which gives for the reduction - - - −0. 0.166

And

	Feet.	In.	Pts.
And hence there remains for the ultimate or true length of the baſe of verification, in the temperature of 62° of FAHRENHEIT's thermometer, being the heat to which that on Hounſlow Heath was reduced, - -	28532.	11.	036

Which make 28532.92 feet.

ART. VIII. *Remarks on the comparative accuracy of the two baſes.*

With regard to the accuracy of the meaſurement of this baſe, compared with that executed on Hounſlow Heath in 1784, from the infinite pains and care beſtowed in both operations, it is very difficult to ſay, to which the preference ſhould be given. The expanſion of glaſs being ſo much leſs than that of ſteel, if manageable glaſs rods of equal length with the chain could have been obtained; then, as far as that ſingle circumſtance might have affected the reſult, a meaſurement made with ſuch glaſs rods would undoubtedly have deſerved the preference to one with the ſteel chain. But when it is conſidered, that the expanſion of ſteel was determined by the pyrometer with the ſame care as that of glaſs; that the wear of the chain is ſo very ſmall, as we have ſhewn it to be, in ſix weeks uſe; that coffers were laid for it, and its length transferred by means of the braſs ſcales to the tops of immovable poſts; that, in the preſent caſe, there was but one-fifth part of the error ariſing from faulty co-incidences as with the twenty-feet glaſs rods; on this view of the matter, the preference ſeems to be due to the meaſurement by the ſteel chain, ſuppoſing always the error in exceſs, cauſed by the deviation from the *allignement* horizontally or vertically, to have affected both equally.

As

As a proof that the expanſion of the chain was accurately determined, I ſhall cloſe this ſection with a remark repeatedly made by the two gentlemen entruſted with the execution of this laſt meaſurement. At the cloſe of each day's work, the two ſcales marking the extremities of the laſt chain (after regiſtering the diviſions of co-incidence) were left upon their reſpective poſts until the next morning. They were ſecured during the night, from being diſturbed by cattle, with a certain number of the ſpare poſts driven into the ground around them. A tent was alſo pitched between the two, where ſome men of the party conſtantly lay, by way of a guard for the whole apparatus. On the recommencement of the operation the ſubſequent morning, the chain being applied anew to the braſs ſcales; if the temperature continued the ſame, the co-incidences were found to be equally accurate as on the preceding night; but if it had changed one or two degrees, the chain never failed unequivocally to ſhew it, by falling ſhort of the diviſions on the ſcales, if the cold had increaſed, or by over-reaching them if it had diminiſhed.

Finally, with reſpect to the ſubject of theſe baſes, it is here to be remarked, that the baſe of verification in Romney Marſh makes with the meridian of the pipe at High Nook an angle of 54° 28′ 56″¼ north-weſtward; and *that* on Hounſlow Heath makes with the meridian of the pipe at Hampton Poor-houſe an angle of 44° 41′ 49″, alſo north-weſtward.

GENERAL

GENERAL TABLE of the Measurement of the BASE of VERIFICATION in ROMNEY
to be $9512\frac{4444}{10000}$ Yards, and the true, or corrected l

Days.	Spaces measured.	Temperature. Mean by 15 Therm.	Temperature. diff. from 62°.	Correction for the difference.	Days.	Spaces measured.	Temperature. Mean by 15 Therm.	Temperature. diff. from 62°.	Correction for the difference.	Days.	Spaces measured.	Temperatur Mean by 15 Therm.	dif from
	Yards.					Yards.					Yards.		
Oct		°	°	In. Parts.	Oct.		°	°	In. Parts.	Nov.		°	
15	100	54.7	− 7.3	0.16710		2100	65.0	+ 3.0	+0.06867		4100	55.2	−
16	200	62.7	+ 0.7	+0.01602		2200	64.1	+ 2.1	+0.04807	10	4200	55.3	−
	300	61.3	− 0.7	0.01602		2300	56.7	− 5.3	0.12132		4300	53.6	−
17	400	57.0	− 5.0	0.11445	30	2400	58.7	− 3.3	0.07554		4400	49.0	−
	500	52.2	− 9.8	0.22432		2500	59.5	− 2.5	0.05722	12	4500	50.1	−
	600	53.6	− 8.4	0.19228	31	2600	57.3	− 4.7	0.10758		4600	47.9	−
20	700	46.8	−15.2	0.34793		2700	54.6	− 7.4	0.16939	13	4700	44.7	−
	800	58.9	− 3.1	0.07096	Nov.	2800	53.9	− 8.1	0.18541		4800	44.8	−
23	900	53.9	− 8.1	0.18541	1	2900	49.0	−13.0	0.29757		4900	41.3	−
	1000	55.3	− 6.7	0.15336		3000	54.0	− 8.0	0.18312	14	5000	41.8	−
24	1100	55.7	− 6.3	0.14421		3100	50.9	−11.1	0.25408		5100	42.9	−
	1200	50.0	−12.0	0.27468	2	3200	49.1	−12 9	0.29528	15	5200	45.3	−
	1300	55.2	− 6.8	0.15565		3300	50.4	−11.6	0.26552		5300	44.1	−
26	1400	59.1	− 2.9	0.06638		3400	48.5	−13.5	0.30901		5400	40.4	−
	1500	60.0	− 2.0	0.04578		3500	42.6	−19.4	0.44407	16	5500	41.5	−
27	1600	59.1	− 2.9	0.06638	5	3600	52.3	− 9.7	0.22203		5600	44.8	−
	1700	63.1	+ 1.1	+0.02518		3700	53.0	− 9.0	0.20601		5700	44.6	−
	1800	68.1	+ 6.1	+0.13963		3800	52.4	− 9.6	0.21974	17	5800	40.6	−
	1900	57.9	− 4.1	0.09385		3900	47.3	−14.7	0.33648		5900	39.4	−
29	2000	60.8	− 1.2	0.02747	7	4000	55.6	− 6.4	0.14650		6000	41.3	
				−2.16540					−3.77913				

From the above Table it appears, that the total apparent length of the Base, as
The corrections in the above Table, and others specified in the Text, being su

There remains, for the true length of the base in the temperature of 62° of Fal

[To face page 134.]

INEY MARSH, executed in the Autumn of 1787, whereby the apparent Length is found
cted Length in the Temperature of 62°, 28532 $\frac{0\,2}{1\,0\,0}$ Feet.

Temperature. Mean by Therm.	Temperature. diff. from 62°.	Correction for the difference.	Days.	Spaces measured. Yards.	Temperature. Mean by 15 Therm.	Temperature. diff. from 62°.	Correction for the difference.	Days.	Spaces measured. Yards.	Temperature. Mean by 15 Therm.	Temperature. diff. from 62°.	Correction for the difference.
°	°	In. Parts.			°	°	In. Parts.			°	°	In. Parts.
.2	— 6.8	0.15565		6100	42.1	— 19.9	0.45551		8100	36.3	— 25.7	0.58827
.3	— 6.7	0.15336	Nov.	6200	39.3	— 22.7	0.51960		8200	39.2	— 22.8	0.52189
.6	— 8.4	0.19228	21	6300	43.3	— 18.7	0.42804	Nov.	8300	39.5	— 22.5	0.51502
.0	— 13.0	0.29757		6400	46.5	— 15.5	0.35479	29	8400	34.8	— 27.2	0.62261
.1	— 11.9	0.27239		6500	45.6	— 16.4	0.37540		8500	40.5	— 21.5	0.49213
.9	— 14.1	0.32275	22	6600	42.5	— 19.5	0.44635		8600	42.3	— 19.7	0.45093
.7	— 17.3	0.39600		6700	42.2	— 19.8	0.45322	30	8700	30.5	— 31.5	0.72103
.8	— 17.2	0.39371		6800	41.2	— 20.8	0.47611		8800	44.3	— 17.7	0.40515
.3	— 20.7	0.47382	23	6900	39.8	— 22.2	0.50816	Dec.	8900	46.0	— 16.0	0.36624
.8	— 20.2	0.46238		7000	39.0	— 23.0	0.52647	1	9000	43.0	— 19.0	0.43491
.9	— 19.1	0.43720		7100	37.7	— 24.3	0.55623		9100	45.6	— 16.4	0.37540
.3	— 16.7	0.38226	24	7200	36.2	— 25.8	0.59056		9200	48.7	— 13.3	0.30444
.1	— 17.9	0.40973		7300	42.1	— 19.9	0.45551	3	9300	41.1	— 20.9	0.47840
.4	— 21.6	0.49442		7400	40.5	— 21.5	0.49213		9400	46.9	— 15.1	0.34564
.5	— 20.5	0.46924	26	7500	35.2	— 26.8	0.61345	4	9512.2454	48.4	— 13.6	0.34942
.8	— 17.2	0.39371		7600	39.8	— 22.2	0.50816					
.6	— 17.4	0.39829		7700	38.5	— 23.5	0.53791					— 6.97148
.6	— 21.4	0.48985	27	7800	33.6	— 28.4	0.65098					— 10.14802
.4	— 22.6	0.51731		7900	38.9	— 23.1	0.52876					— 7.58574
.3	20.7	0.47382	28	8000	32.7	— 29.3	0.67068					— 3.77913
												— 2.16540
		— 7.58574					— 10.14802					
											Total Correction	— 30.64977

					Feet.	In.
Base, as given immediately by the Steel Chain, was 9512.$\frac{2454}{10000}$ yards, which are equal to					28536	8.835
being subtracted from the apparent length,	—	—	—		3	9.799
					28532	11.036
° of Fahrenheit,	—	—	—	—	—	
					equal to —	28532 $\frac{2}{10}$ Feet.

SECTION

SECTION SECOND.

General Deſcription of the great inſtrument with which the angles, in the recent trigonometrical operation, were obſerved; ſhewing alſo its various adjuſtments for practice. Reference to be had to Plate III. a general view of the entire machine; Plate IV. a plan and two ſections; Plate V. various parts repreſented to large ſcales; and Plate VI. the microſcopes and eye-pieces.

ARTICLE I. *Preamble.*

IN endeavouring to deſcribe the curious inſtrument made uſe of for obſerving the angles in the recent trigonometrical operation, it has been judged beſt to confine ourſelves to the principal parts, without entering into any detail of the *minutiæ*: for even to have mentioned theſe, with the almoſt infinite number of little ſcrews that ſerve to unite them into one entire machine, which could only have been done by references to multitude of great and ſmall Roman and Greek characters, would have been a diſguſting labour. By the help of the four plates which this deſcription refers to, and which have been executed with great care, that fewer words might ſuffice, it is hoped, that the inſtrument may be underſtood by two claſſes of people for whom it is chiefly intended; firſt, by thoſe who being poſſeſſed of ſuch a machine would wiſh to make themſelves maſters of its uſe; and, ſecondly, by ſuch ingenious artiſts as would attempt to conſtruct ſuch another; for theſe laſt, in particular, the parts that are of braſs, of bell-metal,

or

or of ſteel, are diſtinguiſhed from each other. And here it is neceſſary to obſerve, that the plates muſt not only be frequently conſulted, but alſo attentively conſidered, and repeatedly compared with each other, in the courſe of this deſcription.

Art. II. *General view of the inſtrument.*

It is a braſs circle, three feet in diameter, and may be called a great theodolet, rendered extremely perfect; having this advantage in particular, which common theodelets have not, that its tranſit teleſcope can be nicely adjuſted by inverſion on its ſupports; that is to ſay, it can be turned upſide down, in the ſame manner that tranſit-inſtruments are, in fixed obſervatories.

The circle is attached by ten conical tubes, as ſo many *radii*, to a large vertical, conical hollow axis of twenty-four inches in height, which may be called the exterior axis. Within the baſe of this hollow axis, a collar of caſt ſteel is ſtrongly driven; and on its top there is inſerted a thick bell-metal plate, with ſloping cheeks, which, by means of five ſcrews, can be raiſed or depreſſed a little.

The inſtrument reſts on three feet, which are firmly united to each other at the place where they branch off, by a ſtrong circular plate of bell-metal, upon which riſes another vertical hollow cone, of leſs ſize than the former, being included within it, and is therefore called the interior axis. On its top is inſerted a caſt-ſteel pivot, with ſloping cheeks, paſſing through the bell-metal plate on the top of the exterior axis, the cheeks of the one being nicely ground to fit the cheeks of the other. The bell-metal baſe of this interior axis is in like manner ground to fit the caſt-ſteel collar in the baſe of that which is without it. Thus the circle being lifted up by two

men

men laying hold of its *radii*, and the exterior being placed upon the interior axis, the cheeks at the top being at the ſame time adjuſted to their proper bearing, it turns round very ſmoothly, and is perfectly, or at leaſt as to ſenſe, free from any central ſhake. This mode of centering is one of the chief excellencies of the inſtrument. From the uſe that has been made of it both years, it ſeems not to have ſuffered in the leaſt; and it is perhaps the only conſtruction that could have anſwered for a machine of ſuch magnitude, undergoing ſo many quick tranſitions from place to place, and ſo often raiſed to high ſituations without any riſk of being thereby hurt.

ART. III. *Mahogany Planes under the inſtrument.*

By inſpection of the plates, but more particularly the IIId, and the ſection towards the right hand in the Vth, it will be ſeen, that there are three planes of mahogany under the metal parts of the inſtrument; namely, that which forms the top of the ſtand, which, although a ſquare of about three feet four inches at bottom, becomes, by the ſeparation of the legs, an octagon at top. In the center there is a circular opening of nine inches diameter, the uſe of which will appear hereafter. Over the top of the ſtand lies another plane of mahogany, likewiſe an octagon, of ſomewhat greater dimenſions than the former, with a circular curb running around it, about half an inch within the planes of its ſides. This octagon hath in its center an open conical ſocket of braſs, three inches in diameter; and on four of its oppoſite ſides there are fixed four ſtrong braſs ſcrews, one on each ſide, which acting againſt pieces of braſs inlaid into the oppoſite ſides of the top of the ſtand, the octagon plane, with every thing that reſts on it, may thereby be moved in four oppoſite directions, until the plummet ſuſ-

pended from the center of the inſtrument above, is accurately brought to co-incide with the point marking the ſtation underneath. The third or uppermoſt plane of mahogany is in fact a part of the inſtrument itſelf, being at all times by ſcrews or otherwiſe united to it, and carrying the handles whereby it is lifted out for uſe, or in again into its caſe, to be tranſported from place to place. In the middle of this plane or bottom to the inſtrument, there is another conical braſs ſocket, of three inches and a quarter in diameter, fitted to ſlip over and turn eaſily on that in the center of the octagon underneath. In the braſs cover of this ſocket, there is a very ſmall hole concentric with the inſtrument, to ſuffer the thread or wire to paſs, which ſuſpends the plummet; and in the view, Plate III. may be ſeen another ſmall box that contains the thread, with a winch-handle for raiſing or lowering the plummet, according as the height of the inſtrument above the ſtation on the ground, or edifice where it ſtands, may require.

ART. IV. *Feet Screws for levelling the inſtrument.*

By attending to the group repreſenting the front elevation of the feet ſcrews, with its ſide nuts, in the right hand upward angle of Plate V. it will appear, that they are ſlackened, which is always the caſe before the inſtrument is levelled, to give room for that operation by the action of the ſcrews. This being done, the ſide nuts are brought to preſs gently on the horizontal plate that embraces the whole group, and thereby keeps the inſtrument as it were united to the mahogany until ſome freſh adjuſtment becomes neceſſary. When the inſtrument is to be put into its caſe, then the feet are let down, and by the ſide nuts the horizontal plate is brought to preſs ſtrongly

on

on the whole group, whereby it is kept perfectly fast and secure from motion in carrying from one situation to another.

ART. V. *Blocks of box wood and conical rollers under the feet screws.*

By referring also to Plate V. it will appear, that directly below each foot there is fixed to the lower surface of the mahogany a small block of box wood, curvilinear in the direction of its motion. On these three blocks rests the whole weight of the instrument, which nevertheless can be moved circularly on them alone. But to render the motion perfectly easy, three conical brass rollers, placed somewhat nearer to the center, are, by means of their respective springs and regulating screws, brought to act and receive such a proportion of the weight as it may be necessary to lay upon them. The head of one of these screws, which give more or less action to the rollers, may be seen at D in the principal view of the instrument Plate III. as well as in the plan and section Plate V.

ART. VI. *Screws giving motion to the whole instrument.*

By examining attentively the general view of the instrument may be seen, in two positions, the great screw with the flat ivory head, whereby the entire machine received a circular motion. In one, it is attached to the curbs, as when in use in 1787; in the other, it is laid upon the mahogany bottom, as was the case the same year every time it was carrried to a new situation. But this ivory-headed screw having been found to act by jerks in moving so great a weight, and consequently to be troublesome in adjusting the instrument to the fixed point, or that of commencement in measuring angles; it was therefore laid aside in 1788, and another apparatus or clamp was

adapted for the ſame purpoſe. This laſt may be ſeen attached to the curbs, as repreſented towards the right hand of Plate V. It conſiſts of a braſs cock, fixed to, and projecting outwards from, the curb of the inſtrument; which cock is acted upon by two ſcrews working on the oppoſite ſides againſt it, and which are clamped to the curb of the octagon.

Art. VII. *Mahogany Baluſtrade and Cover.*

The curb, whereon the three feet of the inſtrument reſt, carries a baluſtrade of mahogany fitted to receive, on the top thereof, a mahogany cover, no where repreſented except in the two ſections in Plate IV. In this cover there are only four ſmall openings (beſides that which allows the great vertical axis to paſs), *viz.* one for each vertical microſcope, one for the clamp of the circle, and one for the ſocket of the Hook's-joint. The two laſt are leſs than the former. At the ſame time that this cover effectually ſecures the circle with its cones from dirt and from accidents, it ſerves conveniently for laying the Hook's-joint upon, or any thing that may be conſtantly wanted near at hand; but more particularly for placing the lanterns uſed at night for reading off the diviſions on the limb of the inſtrument that come immediately under the vertical microſcopes.

Art. VIII. *Achromatic Teleſcopes.*

Two achromatic teleſcopes, each of thirty-ſix inches focal length, with double object-glaſſes of two inches and a half aperture, belong to the inſtrument. They are excellent of their kind, and are furniſhed with eye-pieces of different magnifying powers, for erect as well as inverted viſion. The lower teleſcope lies exactly under the center of the inſtrument, and is directed through one of the openings of the baluſtrade. Being only

only uſed for terreſtrial objects, it requires but a ſmall elevation or depreſſion, and therefore is only ſupplied with a ſhort axis of ſeventeen inches in length, ſupported by braces attached to the feet. The eye end of this teleſcope is purpoſely made heavier than the object end; and reſting on an horizontal arm, that is raiſed or depreſſed by rack-work, it is thereby readily brought to bear, and remain very ſteadily, upon its object. The rack-work may be ſeen in the view of the inſtrument, and alſo on the left ſide of the right hand ſection in Plate IV. But there is a ſmall horizontal motion that can be given to the right hand end of the axis of this teleſcope, which is effected by means of a handle inſerted through the vacancy of the baluſtrades, and placed on a dovetail at E, which could not be ſhewn in the plate. Thus the inſtrument being nicely levelled, the upper teleſcope at zero, and likewiſe on its object, the lower teleſcope, by the help of this adjuſtment, is brought accurately to the ſame object, ſuppoſed to be the point of commencement, or that from which angles are meaſured.

By referring to Plates III. and IV. and likewiſe to the ſection on the left ſide of Plate V. it will be ſeen, that a horizontal bar extends acroſs the top of the vertical axis, ſupported by two ſide braces that ſpring from the cone, about one-third of its height above the plane of the inſtrument. The horizontal bar carries the Y's or ſupports, in which the pivots of the upper teleſcope move. They are of ſuch height as to permit a ſemicircle of ſix inches radius, attached to the axis of the tranſit, to paſs freely, and conſequently the teleſcope to be directed to the ſun or ſtars in high elevations, but not to be brought to the zenith. The arc of exceſs of the ſemicircle likewiſe admits of ſeveral degrees of depreſſion being meaſured thereon.

ART.

Art. IX. *Spirit Levels.*

The inſtrument has two very good ſpirit levels, that are fitted with the ſeveral means of adjuſtment, as is uſual in ſuch caſes, the detail of which it is unneceſſary here to enter into. The firſt or axis level, becauſe it is only applied on the axis of the teleſcope, is that whereby it is ſet horizontal, as in the ordinary tranſit inſtrument; and it is likewiſe uſed for placing the conical axis truly vertical, ſo that the inſtrument may turn round without ſenſible alteration of the level, previouſly to obſervations of the pole ſtar, or of other heavenly bodies.

The ſecond, or elevation level, is that whereby the teleſcope is brought to be truly horizontal, when angles of elevation or depreſſion are to be taken. At ſuch times it is ſuſpended on a rod attached to the outſide of the teleſcope, to whoſe axis of viſion the rod, by adjuſtment, can be made parallel, as will readily be conceived, by obſerving the repreſentation of theſe parts in the right hand ſection of Plate IV.

When the angles of elevation or depreſſion to be determined are very ſmall, they are meaſured by the motion of an horizontal wire in the focus of the eye-glaſs of the teleſcope; but when great, their quantity is meaſured by the arc of motion of the ſemicircle, as ſhewn by its proper horizontal microſcope.

The elevation level is likewiſe made uſe of for levelling the inſtrument when horizontal angles only are to be taken, for which purpoſe it is ſuſpended on two pins, which are ſeen projecting from the horizontal bar in the plan, and one of them in each of the ſections in Plate IV. This was the ordinary poſition of the elevation level when the angles of the

triangles were obſerved, and thereby it was eaſily ſeen in the courſe of the operation, whether the inſtrument had ſuffered any change to render a re-adjuſtment neceſſary.

ART. X. *Lanterns for the Illumination of the Wires.*

The axis of the tranſit teleſcope is hollow, and in the middle there is placed, at an angle of 45° with the axis of viſion, a perforated elliptical illuminator for throwing light on the wires in night obſervations. The light is communicated from a ſmall lantern attached to the horizontal bar at its junction with the brace, directly oppoſite to the end of the axis, which has a bit of thin glaſs placed before it to prevent duſt from entering. There is another ſuch lantern for the lower teleſcope, not however repreſented in the plate. As the light given by theſe lanterns was found to be rather too weak, eſpecially that for the upper teleſcope, therefore it was cuſtomary in practice to illuminate the wires, by holding up frontwiſe one of thoſe ſeen in the ſection in Plate IV. againſt the end of the axis of the upper teleſcope, when directed to the pole ſtar. The ſame method was uſed by preſenting it obliquely to the object-glaſs of the lower teleſcope, when it became neceſſary to examine whether the interſection of the wires continued without ſenſible variation on a reverberatory lamp, commonly placed twelve or fifteen miles off, and ſometimes even at the great diſtance of twenty or twenty-four miles.

ART. XI. *Lanterns for throwing light on the Diviſions of the inſtrument.*

Beſides the two ſmall lanterns for illuminating the wires of the teleſcopes in night obſervations, two larger ones may be ſeen, as already mentioned, ſtanding on the mahogany cover in the

ſection

ſection in Plate IV. uſed for reading off the diviſions of the inſtrument, under the vertical microſcopes. The front of one of theſe is ſhewn, and the back, or that to which the handle is fixed, of the other. Their narrow ſides are preſented towards the microſcopes, there being in each a ſilvered reflector of copper at FF; and oppoſite to it, at GG, a ſcreen of talc or tranſparent oiled paper. The light from a wax candle being thrown on the reflectors, and thence back again through the ſcreens, on the diviſions of the inſtrument under the microſcopes, theſe could be very diſtinctly read off and regiſtered: for the light communicated in this way was very ſtrong, at the ſame time that the glare of it, which otherwiſe would have been diſagreeable to the ſight, was removed by paſſing through the ſcreen.

ART. XII. *Arms projecting from the bell-metal plate under the plane of the inſtrument.*

By referring to Plates III. and IV. but more particularly the latter, it will be perceived, that there are three flat arms, ſtrongly fixed by ſcrews to the edge of the circular bell-metal plate, forming, as has been already mentioned, the baſis of the interior vertical axis. Theſe arms, which are alſo firmly braced to the feet of the inſtrument, riſe gradually as they project outwards towards the circumference of the circle, whoſe radius they exceed about an inch and a quarter, and their extremities are about an inch lower than its upper ſurface. One arm, lying directly over one of the feet, is that to which are attached the wheels and ſcrew moved by the Hook's-joint, and alſo the clamp of the circle, as repreſented in Plate V. The other two arms, whereof one lies alſo over a foot, and the other directly oppoſite to it, become thereby a diameter to the circle,

circle, having their extremities terminated in a kind of blunted triangular figure, forming the baſes of pedeſtals whereon ſtand the vertical microſcopes hereafter to be deſcribed. The arms, together with the horizontal bar and braces carrying the tranſit teleſcope, are every where pierced, in order to leſſen the weight without diminiſhing the ſtrength of the parts.

ART. 13. *Vertical Microſcopes.*

Two vertical microſcopes, diſtinguiſhed A and B, are uſed for reading off the diviſions on the oppoſite ſides of the circle immediately under them. They are exactly of the ſame conſtruction, and the chief parts of that marked A are repreſented in their real dimenſions towards the left hand of Plate VI.; where, beſide the general, may be ſeen particular plans of the ſlides, and alſo that of the pedeſtal, containing within it the gold tongue, with its axis and ſcrews for adjuſtment. Next to theſe plans ſtand the elevation and optical lines, ſhewing the poſition of the glaſſes with the magnified ſcale at the bottom.

Each microſcope contains two ſlides, one lying immediately over the other, their contiguous ſurfaces being in the focus of the eye-glaſſes. The uppermoſt, or that neareſt the eye, is a very thin plate of braſs, to the lower ſurface of which is attached the fixed wire, having no other motion than what is neceſſary for adjuſtment, by the left hand ſcrew to its proper dot, as hereafter to be explained.

The ſteel ſlide immediately under the former is made of one entire piece, of ſufficient thickneſs to permit the micrometer ſcrew, of about 72 threads in an inch, to be formed of it. To its upper ſurface is fixed the moveable wire, which changes its place by the motion of the micrometer head, ſeen in the plan and elevation towards the right hand. The head is divided

into 60 equal parts, each of which repreſents one ſecond of angular motion of the teleſcope. By examining the particular plan of this ſteel ſlide, it will be ſeen, that it is attached by a chain to the ſpring of a watch, coiled up in the uſual manner, within a ſmall barrel adjacent to it in the frame. By this proviſion no time whatever is loſt; the ſmalleſt motion of the head being inſtantly ſhewn, by a proportionable motion of the wire, to one hand or the other, in the field of the microſcope.

It is neceſſary to remark, that the whole microſcope between its pillars can be raiſed or depreſſed a little more or leſs, with regard to the plane of the circle, by the help of two ſteel levers, ſeen one on each ſide of the elevation, which for that purpoſe are applied in the holes repreſented above and below the projecting plate that unites the tops of the pillars. By means of this motion, diſtinctneſs is obtained at the wires; and by the motion of the proper ſcrew of the object lens, which neceſſarily follows that given to the whole microſcope, the ſcale is ſo adjuſted as that fifteen revolutions of the head ſhall move the wire over fifteen minutes, or one grand diviſion, on the limb, equal to nine hundred ſeconds, each degree on the circle being only divided into four parts. This operation being delicate, requires great patience and many repetitions, before the purpoſe can be exactly, or even nearly, effected: for at the ſame time that the fixed wire muſt biſect the dot on the gold tongue, the moveable wire muſt alſo biſect the dot at 180° on the limb, as well as the firſt notch in the magnified ſcale at the bottom of the plate, where the minutes in the field of the microſcope are repreſented in the proportion of between fifteen and ſixteen to one as painted on the eye of the obſerver. In this adjuſtment there is yet another circumſtance to be

attended to, which is, that ſixty on the micrometer head ſhould ſtand nearly vertical, ſo as to be conveniently ſeen. A few ſeconds of inclination to one ſide or the other are of no moment, becauſe the dart or index being brought to that poſition, whatever it may be, muſt at all times remain there without alteration, unleſs ſome derangement that may have happened to the inſtrument, in tranſporting from one place to another, ſhould have rendered a freſh adjuſtment neceſſary. But if, when the wires co-incide with their reſpective dots and the firſt notch, ſixty on the micrometer head ſhould happen to be underneath, or ſo far over from the vertex on either ſide as to be ſeen with difficulty, then the gold tongue muſt be moved a little by means of the capſtan-headed ſcrews, which act againſt each other on the oppoſite extremities of its axis. Thus, by repeated trials, the wiſhed-for object will at length be effected, that is to ſay, ſixty, to which the dart is to be ſet, will ſtand in a place eaſily ſeen. But it is not to be expected, that each microſcope will give juſt nine hundred ſeconds for the run of fifteen minutes. Without great loſs of time this cannot be done; beſides that two obſervers, of different ſights, will adjuſt the microſcopes differently. Accordingly, in 1787, after many trials of the runs in meaſuring fifteen minutes on the different parts of the limb, microſcope A was found to give only 896″, while B gave at a medium 901″. But in 1788, microſcope A gave 900″, while B gave no more than 894″. Theſe differences were of courſe regiſtered and allowed for in the eſtimation of the angles for computation, whereby any difference between them almoſt wholly diſappeared.

The gold tongue, which is extremely thin, applies very cloſely to the ſurface of the circle. In the plan it is ſuppoſed to be ſeen through a thin plate of braſs covering the whole

pedeſtal, and alſo through a ſmall ſquare plate lying over the former, and faſtened to it by three ſcrews. In the under ſide of this laſt, there is a cavity for the projecting part of the tongue. This contrivance of the tongue with its dot was to guard againſt any error that might ariſe from accidental motion given to the inſtrument between one obſervation and another, which from this precaution could never happen, without being immediately diſcovered: for the wires being adjuſted to their dots under the microſcopes reſpectively, if the inſtrument be then turned round 180°, the wires will reciprocally biſect the dots that were originally oppoſite to them, and thereby ſhew, that they are accurately in the diameter of the circle; and ſo on with regard to any other dots whatever. Hence this becomes the moſt ſevere mode of trying the juſtneſs of the diviſions of the inſtrument.

ART. XIV. *Manner of reading off angles with the microſcopes.*

By attending to the magnified ſcale at the bottom of the plate, it will appear, that the dot on the gold tongue, which is here inverted, is about one minute to the left of zero, and alſo of the firſt notch, with which the moveable wire alone co-incides. Now it will eaſily be conceived, from what has been ſaid in this deſcription, how readily, as well as accurately, any obſervation of an angle can be read off with ſuch an inſtrument; for the degrees and quarters, that is to ſay, the 15′, 30′, or 45′, being ſeen with the naked eye, and regiſtered, the value of the fractional ſpace between zero and the laſt paſt grand diviſion, ſeen in the field of the microſcope, is obtained by turning the micrometer head until the moveable wire biſects the dot at that grand diviſion. The number of notches towards the right hand paſſed over on the ſcale, equal to ſo many revolutions

tions of the head, are the number of minutes, always less than 15′, to be added. If there be no odd seconds, the dart will then stand at 60° on the head; but, if any number of seconds are to be added, the dart will shew, by its position with regard to 60°, what that number is. Thus, by adding the parts together, the measure of the total angle is obtained.

The construction, adjustment, and application of these vertical microscopes have been given more fully, because they form a most essential part of the instrument: for the fixed wire constantly remaining on its dot, the fractional space may be repeatedly measured many times over, if necessary, and a mean result may then be taken. But it rarely happens that two observers, reading off with the opposite microscopes, differ more than half a second from each other at the very first reading. If time therefore permits, and the circumstances of the weather should also be favourable for repeating the observation with the telescope, it is sufficiently obvious to what a wonderful degree of accuracy the measure of angles may in this way be obtained.

Art. XV. *Horizontal Microscopes.*

Besides the two vertical microscopes, applied in the manner that has been described to the measurement of the fractional space in horizontal angles, there is yet another to be mentioned, which is placed horizontally on the bar that carries the transit telescope, and is directed to the divisions on the semicircle attached to its axis, for the measurement of angles of elevation or depression, as has already been taken notice of. This microscope, which is of the same construction with the others, but larger, being upwards of nine inches in length, is represented in its full dimension in Plate VI. It has, like the others, a slide made of steel, of such thickness as to permit the

micrometer ſcrew to be formed of it; and it carries a vertical wire placed in the focus of the eye-glaſſes, in which poſition it is moved parallel to itſelf from left to right, by turning the micrometer head. This ſlide is alſo attached to a watch ſpring which acts in a contrary direction to the head, as in other microſcopes of this ſort.

Each degree of the ſemicircle being divided into two parts or 30′, and one revolution of the micrometer head moving the wire in the field of the microſcope 3′; therefore in 10 revolutions it changes its place half a degree or 30′, which are ſhewn by a ſcale of 10 notches in the upper part of the field of the microſcope, and alſo repreſented towards the top of the plate. Each notch correſponds to 3 minutes or 180 ſeconds, and the head being divided into 3 minutes, and each minute into 12 equal parts, therefore each part is of the value of five ſeconds.

ART. XVI. *Concerning the Semicircle.*

With regard to the ſemicircle, which has been repeatedly mentioned in the courſe of this deſcription, it is yet neceſſary to make ſome remarks; and particularly to ſhew how, by its means, the axis of viſion of the teleſcope, when adjuſted, is brought and kept truly horizontal, which is effected in the following manner.

On the oppoſite ſides of the horizontal bar that carries the teleſcope there are fixed four ſmall, but finely poliſhed bell-metal planes, two on each ſide, on the right and left of the top of the vertical axis, in ſuch a manner as that the ſurfaces of the two on either ſide are directed to or in the ſame plane with the center of the axis of the teleſcope. Theſe planes will be beſt conceived

conceived by obſerving attentively the top of the vertical axis in the ſection towards the right hand of Plate IV. On the edge of the ſemircircle may likewiſe be ſeen a moveable clamp, eaſily made to ſlip, with the hand only, around its circumference, and it carries with it a very fine ſteel ſcrew. When the ſemicircle is towards the left hand of the teleſcope, which is its ordinary poſition, the point of the ſteel ſcrew reſts, or may be made to reſt, perpendicularly on the ſurface of the plane that is on the left of the vertical axis. But when the teleſcope is inverted in its Y's, or turned upſide down, as is the caſe in adjuſting the line of collimation, the ſemicircle being then on the right of the teleſcope, and the clamp neceſſarily brought down, the point of the ſteel ſcrew accordingly reſts perpendicularly on the ſurface of the plane to the right of the vertical axis. Thus it will be readily conceived, that in adjuſting the teleſcope by the level for elevations, which is then conſtantly ſuſpended on its proper rod, parallel to the axis of viſion, the action of the ſteel ſcrew on the bell-metal plane ſerves not only for the adjuſtment of the teleſcope in a truly horizontal poſition, for angles of elevation or depreſſion, by the motion of a wire in the focus of its eye-glaſs, in the manner hereafter to be deſcribed, but alſo to keep it in that poſition, by the ſuperior weight of the eye end, rendered ſo on purpoſe. By the ſame means the teleſcope remains ſteadily on any object that it may be directed to for interſection, whether above or below the plane of the horizon.

One thing more with regard to the ſemicircle muſt be mentioned, namely, that it gives angles of elevation 12″ too great, and thoſe of depreſſion 12″ too little. It is very eaſy to conceive, that this aroſe from the impoſſibility of dividing it on the axis of the teleſcope to which it is fixed, and through the centers

centers of whoſe pivots an imaginary line paſſes that ſhould at the ſame time have paſſed through the center of the ſemicircle. Mr. RAMSDEN took the beſt method that could be deviſed to render the excentricity as little as poſſible. Having framed the ſemicircle, and ſcrewed it in its place on the axis, he made a ſteel point firmly fixed to the horizontal bar deſcribe the concentric arcs whereon the diviſions were afterwards to come, and then marked the point for zero, when the teleſcope by adjuſtment had been brought as nearly horizontal as poſſible. Theſe previous ſteps being taken, the ſemicircle was removed, divided on the engine, and replaced in its orginal ſituation. Nevertheleſs, when the inſtrument was carried into the field, and ſcrupulouſly adjuſted, the error was found, as has above been ſaid, 12″, which of courſe became the conſtant quantity to be applied with its proper ſign, when angles of elevation or depreſſion were taken.

ART. XVII. *Eye-glaſſes of the teleſcopes, and mechaniſm of the wires in their foci.*

It has been already mentioned, that the teleſcopes of the inſtrument are furniſhed with eye-glaſſes of different magnifying powers for erect and inverted viſion, ſix for each teleſcope, as follows, *viz.*

	Erect viſion.		Inverted viſion.	
	N°	Power.	N°	Power.
For the lower teleſcope,	N° 1.	58.	N° 1.	43.
	2.	88.	2.	59.
	3.	117.	3.	87.
For the upper teleſcope,	N° 1.	54.	N° 1.	40.
	2.	81.	2.	55.
	3.	108.	3.	80.

With

With regard to theſe eye-glaſſes, it is only neceſſary here to mention, that thoſe of the leaſt magnifying powers were found both in day and night obſervations to anſwer the beſt.

In the focus of the eye-glaſs of the lower teleſcope there are only two wires croſſing each other in acute angles, which are vertical, inſtead of being placed at right angles, horizontally and vertically, as was the ancient method. Since the lower teleſcope never moves through more than a few degrees of a vertical arc, the wires require little or no adjuſtment. Nevertheleſs this was provided for, by allowing room for a ſmall circular motion of the end-piece, which, when adjuſted, is then faſtened by its proper ſcrews, and never afterwards needs any alteration.

By referring to the middle part of Plate VI. two repreſentations of the eye end of the upper teleſcope will be ſeen, with the eye-piece removed. Five wires are ſhewn in this end, namely, two that interſect each other in acute angles, ſimilarly to thoſe in the lower teleſcope; and three that lie horizontally or parallel to each other. Four of theſe, *viz.* the two that form the acute angles, and the two extreme horizontal wires, are fixed in the focus of the eye-glaſſes to the farther ſurface of a thin braſs ſlide, ſuppoſed to be ſeen through the outward braſs, and therefore ſhaded more dark than the reſt. This ſlide, as will be conceived, lies neareſt the eye, and is moveable from right to left, and, *vice verſâ*, horizontally, for the adjuſtment of the line of collimation, by the inſertion of a ſmall mill-head key, on a ſquare pin fitted to receive it, and ſecured by a ſocket on the right hand ſide. The fifth or middlemoſt horizontal wire is attached to the neareſt ſurface of a ſteel ſlide, that lies contiguouſly to, but beyond the former. It is made of one entire thick piece, like thoſe of the micro-

ſcopes, to permit the micrometer ſcrew to be formed of it; and it is repreſented in the uppermoſt figure attached to a watch ſpring coiled up in the uſual manner.

By the motion of the micrometer head, the ſlide, and with it the wire, moves upwards or downwards in the field of the teleſcope, a ſpace equal to half the diſtance of the extreme wires from each other. This motion above or below the central point, which was made to correſpond with the acute interſection of the wires placed in the axis of viſion of the teleſcope, is performed in ten revolutions of the head, as denoted by the motion of the dart, ten diviſions upwards or downwards, in the narrow groove ſeen at the top of the figure.

Now, by the means of this piece of mechaniſm in the eye-end of the teleſcope, it will appear ſufficiently obvious, that ſmall angles of elevation or depreſſion may be determined with great accuracy, when the value of a certain number of revolutions and parts (the circumference of the head being divided into 100) have been once aſcertained by repeated obſervations of the altitude of any well-defined object taken by the ſemicircle. Thus it was found, by experiment, that $7\frac{77}{100}$ revolutions of the micrometer head were equal to an angle of elevation or depreſſion of 10′ 59″, or 659″, on the ſemicircle. Whence it follows, that one revolution raiſes or depreſſes the wire above or below the central point 1′ 24″.8134, or a little more than 84″.81. And hence a motion of one diviſion on the head raiſes or depreſſes the wire nearly $\frac{81}{100}$ths of a ſecond.

In this manner were determined the reciprocal elevations or depreſſions of the ſeveral ſtations of the ſeries of triangles with regard to each other.

By obſerving attentively the four ſcrews repreſented in the outward end of the teleſcope, a dotted groove will be ſeen

under

under the head of each. And in the uppermoſt figure there appears a flat braſs ring, ſoldered to the inſide of the tube about half an inch from the outward end, which carries on its ſurface four ſtuds to receive the lower extremities of the four ſcrews. Thus the grooves allow room for a ſmall circular motion to be given to the end-piece for the vertical adjuſtment of the fork of the wires, thoſe that are horizontal being by conſtruction at right angles with it. This being done, the ſcrews are made very faſt in the ſtuds below, and thereby the whole machinery of the end-piece is rendered perfectly firm and ſecure.

There remains yet one piece more to be barely mentioned. It is the priſm eye-tube, repreſented by dotted lines towards the right-hand ſide of Plate VI. as attached to the eye end of the tranſit teleſcope, inſtead of the common eye-piece with two convex glaſſes. In leaning over our inſtrument to obſerve the pole ſtar, highly elevated in theſe latitudes, the body is neceſſarily thrown into an inconvenient fatiguing poſture, whereby ſome riſk is run of deranging the inſtrument, and conſequently of making the obſervations leſs accurately than when the obſerver can look directly forward, without bending the body ſo much. For this purpoſe, Mr. RAMSDEN promiſed to ſupply the priſm tube in 1787; but it was only, and with great difficulty, obtained in 1788, by which time Mr. DALBY had accuſtomed himſelf to obſerve very well without it, ſo that it was never uſed.

By employing this piece, light is no doubt loſt; becauſe the image paſſes through more glaſſes before it reaches the eye, than when the common eye-piece is uſed. But for obſervations of ſtars nearer the zenith than the pole ſtar is in our latitudes, it would be indiſpenſably neceſſary. It would likewiſe be advantageouſly

uſed in looking at the meridian ſun in ſummer, for which purpoſe it is furniſhed with dark glaſſes, placed in a ſlide moved by rack-work, as may be ſeen from inſpection of the plate. They conſiſt of three priſms, laid cloſe to each other, ſo as to form, when thus aſſembled, a parallelopipid. Here the green priſm ſtands neareſt to the eye, a dark one fartheſt from it, and between the two, one of white flint glaſs, for correction of the refraction which would otherwiſe take place. It will eaſily be conceived, from the diſpoſition of the priſms, that the darkeſt medium is here towards the left; and that it becomes gradually lighter towards the right hand, where a void part in the frame is brought into the field when the ſtars are obſerved; or when, from the circumſtances of the weather, it may be unneceſſary to ſcreen the eye from the ſun's rays.

ART. XVIII. *General management of the inſtrument for obſervation.*

When the inſtrument is uſed on the ground, it is covered from the weather, under a circular tent, eight feet in diameter. Four ſhort piles, hooped and ſhod with iron, are driven into the earth, and their heads levelled, by laying acroſs from one to the other a mahogany ſtraight ruler, having a ſpirit level attached to one ſide of it. The feet of the ſtand being then placed on piles, are firmly faſtened to them by means of long ſquare-headed ſcrews, only one of which may be ſeen in the view of the inſtrument, belonging to that foot which ſtands neareſt the eye. By working with the four ſcrews fixed in the octagonal mahogany plane, the plummet ſuſpended from the center of the inſtrument is brought accurately over the point on the ground that marks the ſtation. The ſcrews of the feet, with the ſide nuts appertaining to them, are then ſlackened,

to

to give ſufficient room for the adjuſtment of the inſtrument, which by them is brought to be level.

ART. XIX. *Adjuſtment of the axis Level.*

The axis of the upper or tranſit teleſcope being brought over any one of the feet, and the circle being clamped, hang the axis level on the pivots or *anſæ* of the teleſcope, and bring the bubble to the two indexes; then reverſe the level, that is, turn it end for end, and note the difference. Biſect this difference, one half by the level's proper adjuſting ſcrew, and the other half by that foot-ſcrew only which is in a line with the axis. This operation being repeated until the difference wholly vaniſhes, the level will be truly adjuſted, that is to ſay, the bubble will reſt between the ſame points in both poſitions.

ART. XX. *Adjuſtment of the elevation Level.*

This level being ſuſpended on the rod attached to the outſide of the tranſit teleſcope, ſcrew the erect eye-tube on, to make that end preponderate. Adjuſt the bubble to the indexes by the ſteel finger-ſcrew at the tail of the ſemicircle's clamp. Reverſe the level, and note the difference. Then biſect that difference, and correct one half by the finger ſcrew, and the other half by the proper adjuſting ſcrew under the level, and ſo on repeatedly until the difference wholly vaniſhes. The level may then be hung on the two pins that project from the horizontal bar which carries the teleſcope, where, being parallel to the axis level, it will ſhew when that is removed (as is commonly the caſe when terreſtrial objects only are obſerved) whether the plane of the inſtrument ſuffers any alteration. If this ſhould have happened, the level on the horizontal bar is at all times ſufficient to correct it.

ART. XXI. *To ſet the vertical Axis perpendicular.*

This may be done by either level, but beſt with the axis level, which being ſuſpended on its pivots, muſt be brought parallel with two of the feet of the inſtrument; and by the ſcrews of theſe two feet, the bubble is to be brought between its indexes. The circle being then turned round 180°, if the bubble changes its place, half the difference is to be corrected by one of the feet ſcrews, and the other half by two capſtan-headed ſcrews, that act againſt each other, under and belonging to one of the Y's, or ſupports, in which the pivots reſt. When the bubble is found to be juſt in theſe two poſitions, turn the circle 90°, which will neceſſarily bring the axis over the third foot of the inſtrument. Then correct any error there may be by that foot ſcrew. In this manner the circle will be made to revolve again and again, without any alteration whatever of the bubble, which ſhews that the vertical axis is then truly perpendicular to the horizon.

ART. XXII. *To make the line of Collimation in the teleſcope at right angles with the tranſverſe Axis.*

The pivots reſting in their Y's, direct the teleſcope to ſome diſtant well-defined object, and let the circle be clamped. Then reverſe the axis, that is, turn the teleſcope upſide down. If the interſection of the wires does not co-incide with the object in both poſitions, half the difference muſt be corrected by the motion of the circle with the Hook's-joint, and the other half by the motion of the braſs ſlide in the eye end of the teleſcope, by applying the milled-head key in the ſmall ſocket ſeen on the right hand ſide in Plate VI. and ſo repeatedly until the difference wholly diſappears.

ART. XXIII. *To ſet the Rod on which the elevation level hangs parallel to the line of Collimation.*

The vertical axis being ſuppoſed to be nearly vertical, hang the level on its rod, and rectify the bubble by the finger ſcrew of the clamp. Set the horizontal wire on the ſteel ſlide, to interſect the center of the oblique wires, and place the dart or index at zero on the micrometer head. Then obſerve ſome diſtant diſtinct object covered by the horizontal wire. Invert the ſemicircle, that is, turn the azimuth circle 180°, and the teleſcope upſide down, ſo as to bring the wire upon or nearly upon the ſame object. Now, if the level be not right, rectify it by the finger ſcrew at the tail of the clamp. If the teleſcope does not now accurately cover the ſame object as in the former poſition, biſect the difference by the finger ſcrew of the clamp, and then rectify the bubble by the capſtan-nuts under one end of the rod. Repeat this operation until the level is right, when the teleſcope ſees the ſame objects in both poſitions, and thereby the rod will be brought parallel in altitude to the line of collimation or axis of viſion.

The adjuſtments of the microſcopes having been already ſufficiently explained, in giving the deſcription of the eſſential parts of the inſtrument, it is unneceſſary here to repeat them.

ART. XXIV. *Of the weight of the inſtrument, and mode of tranſporting it from place to place.*

The inſtrument, whoſe deſcription and uſes we have here attempted to give in a general way, without reference to its minute parts, by a multitude of different characters, weighed in the whole about 200 lbs. It is contained in two deal boxes; one

one of a circular form for the body of the inſtrument; and the other of an oblong ſquare figure, for the tranſit teleſcope. Within this laſt box there is one of mahogany, that holds all the ſmaller parts of the apparatus. The ſtand, ſteps, ſtools, pullies, ropes, tent, and canopy for the ſcaffold, &c. &c. weighed at leaſt as much more. The whole attirail was tranſported from place to place, in a four-wheeled ſpring carriage, drawn by two, and ſometimes by four horſes. The carriage part, originally that of a crane-necked phaeton, was preſented, with his uſual liberality, by Sir JOSEPH BANKS; and upon it was built a kind of caravan, covered with painted oil-cloth, whereby every thing within was kept dry and ſecure.

SECTION THIRD.

Deſcription of various articles of machinery made uſe of in the trigonometrical operation referred to in Plate VII. Alſo the diſtinction of the ſtations into two ſets, thoſe of the ſecond ſet being referred to in Plate VIII.

ARTICLE I. *Portable Scaffold.*

IN the account of the meaſurement of the baſe on Hounſlow Heath we have ſhewn, that the ſurface of that remarkable plain is not elevated more than fifty or ſixty feet above the mean level of the ſea. From this ſmall elevation, and the circumſtance of its being ſurrounded, almoſt on every ſide, with lofty trees, it was from the beginning ſufficiently obvious, that, in order to be enabled to make the obſervations of the collateral

ſtations from the extremities of the baſe, it would be abſolutely neceſſary to raiſe the inſtrument, by ſome means or other, to a conſiderable height above the ground. For this purpoſe the portable ſcaffold, whoſe plan and elevation are repreſented on the left hand ſide of Plate VII. was conſtructed. It conſiſted, as may be ſeen, of an inward ſcaffold for ſupporting the inſtrument, and an outward one for the obſervers, wholly free and independent of each other, the platforms of both being framed about thirty-two feet above the lower ends of the ſcantlings, which reſt on the ground. Theſe being made of ſquared deal, and the ſeveral parts being bolted and ſcrewed together with many iron ſcrews ſecured by nuts, the whole could be readily taken to pieces, carried in a waggon (for which it made a complete load), and replaced again in any new ſituation. This ſcaffold anſwered very well the purpoſe for which it was intended; for the ſtep-ladders, or ſtairs leading to the platform, being attached to the outward frame, the inward one that carried the inſtrument remained undiſturbed by the motion of thoſe who went up and down, or walked around the top. The ſilk thread, that ſuſpended the plummet, was ſecured from the effects of the wind by a ſort of funnel or trunk, compoſed of three deals (one ſide being left open), and ſo contrived as to be eaſily turned round to any quarter of the heavens, whereby the open ſide was always preſented to leeward. The inſtrument was covered from the weather by a canvas canopy, about ſeven feet ſquare, to which ſide walls could be hooked for ſcreening it from the wind, as occaſion might require. By referring to the elevation it will be ſeen, that the ſcaffolds, both outward and inward, might be divided horizontally into two parts, ſo as to permit the uppermoſt half alone to be uſed when it became unneceſſary to raiſe the inſtru-

ment to a greater height than fifteen or ſixteen feet above the ground. The whole together was never made uſe of, except at the two extremities of the Hounſlow Heath baſe. The uppermoſt half was applied at three of the ſtations only, namely, St. Ann's Hill, Botley Hill, and Padleſworth near Dover.

Art. II. *Tripod Ladder.*

Next to the ſcaffold the plate repreſents, in plan and ſection, a tripod ladder, about thirty-five feet in height. It carries on its top a globe lamp, of about one foot in diameter, in which was uſed a ſimple Argand's burner, of a large ſize, made for that purpoſe. The lamp being removed, a ſocket for a white light might occaſionally be ſubſtituted in its place; or (as was the caſe when we obſerved the ſtation at King's Arbour from St. Ann's Hill) a flag-ſtaff might be added at the top, which was ſecured in a truly vertical poſition, by braces fixed to the legs of the ladder underneath. It will be readily conceived, that by a contrivance of this ſort a white light could be raiſed to a conſiderable height above the ground, if the circumſtances at any time had rendered ſuch elevation neceſſary; and that it could, by the help of a heavy plummet, be always placed in a truly vertical poſition over the point on the ground marking the ſtation. The globe lamp was found to anſwer very well for ſhort diſtances of ſix or eight miles, when the weather was favourable; but it could not be depended upon in obſervations of diſtances that were conſiderably greater.

Art. III. *Common Flag-ſtaff.*

After the tripod ladder, comes in the plate the plan and elevation of a common flag-ſtaff with its braces, carrying

likewiſe two reverberatory lamps. Theſe two were attached to the ſame iron bar, at the diſtance of three feet from each other. They had concave copper reflectors, nine inches in diameter, extremely well poliſhed and ſilvered. They were intended at firſt for experiments near London, and were very well ſeen at the diſtance of fifteen or ſixteen miles. To ſecure us from any uncertainty that might have ariſen, by miſtaking other lights for our own, one lamp was placed over the other. But when we came afterwards to be better acquainted with the appearance of theſe lamps, that precaution was found to be entirely unneceſſary; wherefore ſingle reverberatories were provided, with *ſpecula* of ten inches diameter, and they were ſupplied with ſtill larger burners, which could be ſeen at the diſtance of twenty or twenty-four miles. But here it is proper to remark, that theſe lamps muſt be carefully watched, eſpecially in expoſed windy ſituations; for if the cotton be drawn out a little too far, they are apt to ſmoke, whereby the front glaſs becomes obſcure, and therefore muſt be wiped frequently. They are eaſily turned on the poſts that ſupport them; and were, by the help of a teleſcope laid on one ſide, parallel to the axis of the rays (for which a contrivance was provided in the tin work) accurately preſented towards the ſtation occupied by the inſtrument at the time from whence they were to be obſerved. There was conſtantly one of theſe lamps, and ſometimes two, at two different ſtations, burning each night, when we were making obſervations of the pole ſtar, or white lights of ſhort duration, placed at other diſtant ſtations.

ART. IV. *Tripod for White Lights.*

Next after the flag-ſtaff (whereon a ſocket for white lights could likewiſe be placed, when the flag itſelf was removed) is repreſented a ſmall tripod intended for white lights only. The ſame ſocket that fitted the top of the flag-ſtaff, or lamp-poſt, could be applied to the tripod, by the help of three ſmall ſockets ſoldered for that particular purpoſe to the ſides of the principal one. Deal rods, of five or ſix feet in height, or hazels cut from the neareſt hedge, ſerved as the legs of this ſtand. The ſockets themſelves were made of copper, becauſe thoſe of iron would have been diſſolved by the ſulphur; and the upper part, which was only an inch, or an inch and an half, in height, was ſquare or round, according to the figure of the boxes containing the compoſition, ſometimes of one kind, and ſometimes of the other. Theſe white-light tripods, being readily placed by the help of a plummet over the point marking the ſtation, were found to be very convenient on the top of an open hill, or on the leads of a church ſteeple, as the perſon attending them could eaſily light the box with the port-fire, without the aid of a ladder.

ART. V. *Portable Crane.*

On the right hand ſide of the plate is repreſented, in plan and ſection, and by a larger ſcale than the others, a portable crane for weighing up the inſtrument to the tops of ſuch towers, church ſteeples, or other buildings, as became ſtations in the ſeries of triangles. It was conſtructed in the Tower of London, and anſwered very well the purpoſe for which it was intended, although it might ſtill be improved. Before we were

were ſupplied with this crane, we made ſhift, by the help of a long beam, and a moveable treſtle by way of fulcrum for it to reſt upon, to get the inſtrument up to the top of its own proper ſcaffold, and one that was ſtill higher, erected over the tranſit room of the Royal Obſervatory at Greenwich.

ART. VI. *Reaſons for changing certain Stations.*

In the courſe of the trigonometrical operation, the center of the inſtrument has conſtantly been brought, even almoſt to mathematical exactneſs, over the preciſe point marking the ſtation, whereby reductions to the center on account of excentricity have been avoided; and the ſtations have been diſtinguiſhed, as far as poſſible, by permanent marks in ſuch a manner, that, while theſe remain, the center of this or any other inſtrument may be again brought into the ſame vertical line. By theſe means our recent obſervations may be repeated on any future occaſion, and connected with others, which it is to be hoped will be made hereafter: for this operation, the firſt of its kind in Britain, ſhould only be conſidered as the foundation or commencement of a ſeries of others, which by degrees will be carried to the remoteſt parts of the iſland.

By compariſon of the annexed plan of the triangles with that communicated to the Royal Society in 1787, as only a ſketch of the ſcheme then propoſed to be carried into execution, it will be perceived, that ſome few ſtations are omitted entirely, and others ſubſtituted in lieu of ſome that were then intended to be occupied. Of this laſt number Hanger-hill Tower has been made uſe of inſtead of Kew Pagoda. This laſt had been propoſed on a ſuppoſition, that without a ſcaffold of an enormous height, it would have been impoſſible to ſee Hanger-hill Tower from King's Arbour. Nevertheleſs, after

a good deal of trouble, by cutting off the tops of certain trees, lopping the branches of others, and raiſing a flag-ſtaff on the center of the ſcaffold, theſe two ſtations were rendered reciprocally viſible. By theſe means we not only avoided making uſe of Kew Pagoda, which, from the nature of the building, would have been a very incommodious ſtation; but we thereby got rid of Clermont Tower altogether; and thus, inſtead of two ſmall triangles, one was conſtituted, larger and better, being nearly equilateral.

In the introduction there has been occaſion to take notice of the advantage that was gained by being able to ſee Frant and Fairlight Down reciprocally. From this circumſtance the ſeries from Frant eaſtward to the baſe of verification becomes in reality a double one, and conſequently affords better means of aſcertaining the correctneſs of the work.

The ſingularity of the ſituation of Dover Caſtle has likewiſe been mentioned. Inſtead of two ſtations near Tatterlees Barn and Barefriſtan, whereby it was hoped, that Dover Caſtle might have been connected with the ſeries to the weſtward, it was found neceſſary to make uſe of three ſtations; one at Padleſworth, one at Folkſtone Turnpike, and a third at Swingfield. Thus the ſide which connects that ancient fort with the other triangles is ſhorter than was intended. But with ſuch an inſtrument as ours, and where all the angles of the triangles were obſerved, no uncertainty ariſes on that account.

Art. VII. *Diſtinction of the Stations.*

Having aſſigned the reaſons that rendered it eligible or neceſſary to change ſome few of the ſtations propoſed in the original ſcheme, it only now remains to enumerate the whole as diſtinguiſhed

diſtinguiſhed into two ſets. Firſt, thoſe which are permanently marked by pipes ſunk in the earth; and, ſecondly, thoſe where the inſtrument was elevated to the top of ſome tower, church ſteeple, or other building. The plans of the platforms of this laſt ſet are given in Plate VIII. along with ſuch dimenſions as are neceſſary to ſhew, with regard to the ſide walls, the preciſe ſpot over which the center of the inſtrument was placed. As often as was poſſible, theſe ſituations were further defined, by means of concentric circles deſcribed on the leads.

The ſtations of the firſt ſet, marked with pipes, are fourteen in number, *viz.*

Hampton Poor-houſe,	the extremities of Hounſlow Heath baſe.
King's Arbour,	
St. Ann's Hill, .	about the middle on the eaſt edge.
Hundred Acres, .	near the weſt end of the garden.
Norwood, . . .	towards the Croydon end of the heights.
Botley Hill, . . .	in a field belonging to Limpsfield Lodge Farm.
Wrotham Hill . .	in a field belonging to Mr. JOHNSTON.
Hollingborn Hill, .	in a field belonging to Mr. DUPPER.
Fairlight Down, .	347 feet ſouthward from the Windmill, which makes with Fairlight Church, an angle of 105° 53′ 20″.
Ruckinge, . . .	the extremities of the baſe of verification.
High Nook, . .	
Allington Knoll, .	an artificial mount belonging to Sir JOHN HONEYWOOD.
Padleſworth, . .	eaſtward from the Church, in the Broom-field belonging to Mr. BROCKMAN.

Folkſtone

Folkſtone Turnpike . . weſtward from the Public-houſe.

The ſtations of the ſecond ſet, where the inſtrument was elevated on buildings, are nine in number, *viz.*

Hanger-hill Tower.
Tranſit-room of Greenwich Royal Obſervatory.
North-weſt turret of Severndroog Caſtle, on Shooter's Hill.
Swingfield Church Steeple.
North turret of the Keep of Dover Caſtle.
Lydd Steeple.
Tenterden Steeple.
Goudhurſt Steeple.
Frant Steeple.

SECTION FOURTH.

Calculation of the ſeries of triangles extending from Windſor to Dunkirk, whereby the geodetical diſtance between the meridians of the Royal Obſervatories of Greenwich and Paris is determined. Reference to be had to Plate IX.

ARTICLE I. *Exceſs of the angles of ſpherical above thoſe of plane Triangles.*

IF the earth, or any conſiderable portion of its ſurface, was a perfect plane, an inſtrument, ſuch as has been formerly deſcribed, when applied on that ſurface, to determine by trigonometrical meaſurement the extent of the plane part, would every where have its axis parallel to itſelf; and the ſum of the three

three angles of each of the triangles, into whatever number, great or ſmall, it might be divided, would conſtantly amount to 180°. But the earth being a ſphere or ſpheroid, it follows, that the ſame inſtrument, ſucceſſively adjuſted at each of the ſtations, will have its axis perpendicular, on a ſphere, to an equally curved ſurface; on a ſpheroid, to one unequally curved, in either caſe forming the horizon of the ſtation; and the ſum of the three angles of ſuch a ſpherical or ſpheroidical triangle muſt, as is known, always exceed 180°, leſs or more, in proportion to the lengths of the ſides. When the triangles are very ſmall, the exceſs being of courſe ſmall cannot poſſibly be diſcernible by common inſtruments. Even the fineſt, ſuppoſing them free from error of diviſion, will ſcarcely render it perceptible, without the utmoſt care in making the obſervations. This will be ſufficiently exemplified in the following calculations, where a column is inſerted containing the ſpherical exceſs; and another for the difference or error between that and the exceſs of the ſum of the obſerved angles above 180°. From theſe it will appear, that, notwithſtanding the goodneſs of our inſtrument, and the pains taken in uſing it, we have frequently failed in bringing out an exceſs; and indeed the reſults have even ſometimes been in a ſmall degree defective.

It had been at firſt propoſed to multiply the obſervations as much as poſſible, and particularly by ſucceſſively changing the zero of the inſtrument to new points (Phil. Tranſ. 1787, p. 219.), to meaſure the ſame angles on different parts of the circle, ſo as to ſubdivide any errors that might ariſe from inaccuracy of diviſion, or ſhake at the center. This principle, perfectly good in theory, and which was adhered to as far as the circumſtances would permit, was nevertheleſs found, on many occaſions, to be impoſſible in practice, without ſacri-

ficing much more time than we could afford, conſiſtently with the engagements entered into with the French Gentlemen, for the co-operation on the Coaſt. At particular times, eſpecially in hot weather, there was ſuch a tremulous motion or boiling in the air, that it was only during a very ſhort ſpace, chiefly in the mornings and evenings, that the objects were ſufficiently diſtinct to be obſerved with accuracy. So difficult it is to do any thing perfectly good in this way, that a whole day has frequently been ſpent, after watching with anxious care, in obtaining a ſingle one that was perfectly ſatisfactory! At ſuch times as theſe it would have been abſurd to have attempted to change the zero, which always rendered it neceſſary to re-adjuſt the inſtrument by its levels.

In very favourable circumſtances of the weather a good obſervation by day is preferable to one by the white lights at night; becauſe, in the firſt caſe, the obſerver has time at his leiſure nicely to biſect a fine flag-ſtaff, and repeatedly to read off the angle; whereas, in the ſhort duration of the burning of the light, he is ſomewhat hurried, from the fear of loſing ſome of the lights at other diſtant ſtations, if two of them happened to come together, which now and then they did, from the irregularity of the rates of the watches of the artillery-men attending at the different ſtations. It was, however, by the aſſiſtance of the white lights only, that the moſt diſtant ſtations could be rendered viſible; and there cannot be a doubt that, in great trigonometrical operations of this ſort, they will be univerſally adopted hereafter.

Sometimes an obſervation has been entirely loſt, or at leaſt that which had been obtained was not thought a very good one. In ſuch caſes a blank has been left in the column of obſerved angles, and alſo in that expreſſing the error. But no

bad conſequence has ariſen on that account, there being always ſuch other checks from the collateral ſtations, as to leave nothing doubtful.

On the whole, although, for the reaſons already aſſigned, we have repeated the obſervations ſeldomer than was at firſt propoſed; yet it will obviouſly appear from the reſults, and particularly from the near agreement between the meaſured and computed length of the baſe of verification, that a few very good obſervations are greatly preferable to a mean that might perhaps have been obtained of many made in a hurry, which at beſt would have been but indifferent.

The quantity by which the ſum of the three obſerved angles of ſpherical triangles ſhould have exceeded 180° was found as follows.

Becauſe the exceſs of the three angles of a ſpherical triangle above 180° × earth's radius = its area, therefore $\frac{\text{Area}}{\text{Earth's rad.}}$ = exceſs above 180° in ſeconds, if the area and radius are taken in ſeconds. Now, 60859.1 fathoms being = 1° on a mean ſphere, we get the log. of the feet in a ſecond = 2.0061743, and twice this, or 4.0123486 is the log. of the ſquare feet in a ſquare ſecond. Therefore log. area in feet − 4.0123486 = log. area in ſeconds; and the log. of the earth's radius in ſeconds being 5.3144251, we have area in feet − 4.0123486 − 5.3144251 = log. area in feet − 9.3267737 = log. exceſs in ſeconds; that is to ſay, *from the logarithm of the area of the triangle taken as a plane one, in feet, ſubtract the conſtant logarithm* 9.3267737, *and the remainder is the logarithm of the exceſs above* 180° *in ſeconds nearly.*

ART. II. *Calculation of the Triangles.*

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spherical exceſs.	Diff. or error.	Angles corrected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
I.	Hanger-hill Tower	42 2 32			42 2 34	
	Hampton Poor-houſe	67 55 39			67 55 39	
	King's Arbour .	70 1 48			70 1 47	
		179 59 59	0.29	−1.29		
	The BASE between Hampton Poor-houſe and King's Arbour . .				. . . .	27404.7
	Hanger-hill Tower from { Hampton Poor-houſe				. . . .	38461.12
	Hanger-hill Tower from { King's Arbour .				. . . .	37922.57
II.	St. Ann's Hill .	44 18 51.5			44 18 51.5	
	Hampton Poor-houſe	61 26 33.1			61 26 33.5	
	King's Arbour .	74 14 35			74 14 35	
		179 59 59.6	0.21	−0.61		
	St. Ann's Hill from { Hampton Poor-houſe				. . . .	37754.25
	St. Ann's Hill from { King's Arbour . .				. . . .	34455.8

Hence, in the quadrilateral formed by *Hampton Poor-houſe*, *King's Arbour*, *Hanger-hill Tower*, and *St. Ann's Hill*, making uſe of the two obtuſe angles, as contained within their reſpective known ſides, we have for the mean diſtance of the points of the acute angles at Hanger-hill Tower and St. Ann's Hill, expreſſed by a dotted line in the plan of the triangles, 68897.165 feet.

III.	Wardrobe Tower of Windſor Caſtle	. . . . .	. .	. .	58 9 58.5	
	King's Arbour .	62 40 27.5			62 40 27.5	
	St. Ann's Hill .	59 9 14			59 9 14	
			0.25			
	Windſor Caſtle from { King's Arbour .				. . . .	34819.4
	Windſor Caſtle from { St. Ann's Hill .				. . . .	36032.37

N°

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spherical exceſs.	Diff. or error.	Angles corrected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
IV.	Hundred Acres	53 58 35.75			53 58 36.5	
	Hanger-hill Tower	68 24 44			68 24 44	
	St. Ann's Hill	57 36 39.5			57 36 39.5	
		179 59 59.25	1.08	−1.83		
	Hundred Acres from Hanger-hill Tower					71934.2
	Hundred Acres from St. Ann's Hill					79211.22
V.	Severndroog Caſtle, Shooter's Hill	53 31 10			53 31 9.75	
	Hanger-hill Tower	55 53 44.3			55 53 44	
	Hundred Acres	70 35 6.75			70 35 6.25	
		180 0 1.05	1.18	−0.13		
	Severndroog Caſtle from Hanger-hill Tower					84376.68
	Severndroog Caſtle from Hundred Acres					74077.66
VI.	Norwood	107 53 37			107 53 35.75	
	Hanger-hill Tower	26 12 22.5			26 12 23	
	Severndroog Caſtle	45 54 1.5			45 54 1.25	
		180 0 1	0.44	+0.56		
	Norwood from Hanger-hill Tower					63673.31
	Norwood from Severndroog Caſtle					39155.15
VII.	Norwood	88 5 58			88 5 58.07	
	Hanger-hill Tower	29 41 20.75			29 41 21	
	Hundred Acres	. . . .			62 12 40.93	
			0.53			
	Norwood from Hundred Acres					35648.21
VIII.	Tranſit Room, Greenwich Obſervatory	111 56 50			111 56 50	
	Severndroog Caſtle	47 48 14			47 48 13	
	Norwood	20 14 58			20 14 57	
		180 0 2	0.01	+1.9		
	Greenwich Obſervatory from Severndroog Caſtle					14610.58
	Greenwich Obſervatory from Norwood					31274.48

N°

N° of triangles.	Names of the stations.	Observed Angles.	Spherical excess.	Diff. or error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	° ′ ″	Feet.
IX.	Botley Hill	74 37 17.5			74 37 18	
	Hundred Acres	66 0 56.2			66 0 56	
	Severndroog Caſtle	39 21 46.25			39 21 46	
		179 59 59.95	0.78	−0.88		
	Botley Hill from Hundred Acres				. . . .	48726.75
	Botley Hill from Severndroog Caſtle				. . . .	70104.76
X.	Wrotham Hill	54 25 1			54 25 1.25	
	Botley Hill	67 53 11			67 53 10.25	
	Severndroog Caſtle	57 41 49			57 41 48.5	
		180 0 1	1.12	−0.12		
	Wrotham Hill from Botley Hill				. . . .	72953.12
	Wrotham Hill from Severndroog Caſtle				. . . .	79962.13
XI.	Frant	50 19 19			50 19 18	
	Botley Hill	57 15 11.25			57 15 11	
	Wrotham Hill	72 25 31,2			72 25 31	
		180 0 1.45	1.3	+0.15		
	Frant from Botley Hill				. . . .	90364.16
	Frant from Wrotham Hill				. . . .	79723.57
XII.	Hollingborn Hill	. . . . .			47 18 59	
	Wrotham Hill	84 12 24.5			84 12 23.5	
	Frant	48 28 37.5			48 28 37.5	
			1.52			
	Hollingborn Hill from Wrotham Hill				. . . .	81196.58
	Hollingborn Hill from Frant				. . . .	107897.5
XIII.	Fairlight Down	48 25 53.5			48 25 55	
	Frant	79 23 3			79 23 2	
	Hollingborn Hill	. . . . .			52 11 3	
			2.85			
	Fairlight Down from Frant				. . . .	113928.2
	Fairlight Down from Hollingborn Hill				. . . .	141547.1

N°

N° of triangle.	Names of the ſtations.	Obſerved angles.	Spheri-cal exceſs.	Diff. or error.	Angles cor-rected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
XIV.	Goudhurſt	35 26 32.5			35 26 34.5	
	Botley Hill	40 4 42			40 4 42	
	Wrotham Hill	104 28 44			104 28 43.5	
		179 59 58.5	1.35	−2.85		
	Goudhurſt from Botley Hill					121809.3
	Goudhurſt from Wrotham Hill					80997.43
XV.	Goudhurſt	72 23 32.5			72 23 33.87	
	Frant	75 33 16			75 33 13.63	
	Wrotham Hill	32 3 12.8			32 3 12.5	
		180 0 1.3	0.81	+0.49		
	Goudhurſt from Frant					44389.68
XVI.	Hollingborn Hill	63 46 44			63 46 47	
	Wrotham Hill	52 9 11.5			52 9 11	
	Goudhurſt	64 4 3.5			64 4 2	
		179 59 59	1.22	−2.22		
	Hollingborn Hill from Goudhurſt					71296.03
XVII.	Tenterden	67 7 55			67 7 56.46	
	Goudhurſt	68 13 21			68 13 19.5	
	Hollingborn Hill	. . . . .			44 38 44.04	
			0.85			
	Tenterden from Goudhurſt					54374.66
	Tenterden from Hollingborn Hill					71855.0
XVIII.	Fairlight Down	. . . . .			35 20 58.42	
	Goudhurſt	49 39 34			49 39 35.77	
	Tenterden	94 59 26			94 59 25.81	
			0.91			
	Fairlight Down from Goudhurſt					93625.92
	Fairlight Down from Tenterden					71634.73

N°

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spheri-cal exceſs.	Diff. or error.	Angles cor-rected for calculation.	Diſtances
		° ′ ″	″	″	° ′ ″	Feet.
XIX.	Allington Knoll .	48 24 38			48 24 39	
	Hollingborn Hill	. . . .			40 0 58.96	
	Tenterden .	91 34 23			91 34 22.04	
			1.05			
	Allington Knoll from { Hollingborn Hill				. . . .	96036.45
	Allington Knoll from { Tenterden .				. . . .	61775.34
XX.	Lydd .	. . . .			63 14 9.82	
	Allington Knoll .	73 0 27.5			73 0 27	
	Tenterden .	43 45 22			43 45 23.18	
			0.67			
	Lydd from { Allington Knoll				. . . .	47849.27
	Lydd from { Tenterden .				. . . .	66160.93
XXI.	Fairlight Down .	54 59 18.5			54 59 17.31	
	Lydd .	. . . .			62 27 50.18	
	Tenterden .	62 32 53			62 32 52.51	
			0.99			
	Fairlight Down from Lydd				. . . .	71689.73
XXII.	Allington Knoll .	32 59 22.5			32 59 23	
	Lydd .	125 42 0.25			125 42 0	
	Fairlight Down .	. . . .			21 18 37	
			0.33			
	Allington Knoll from Fairlight Down				. . . .	106922.5
XXIII.	Lydd .	43 20 48.25			43 20 48.5	
	Ruckinge .	48 58 49.75			48 58 49.5	
	High Nook near Dym-church .	87 40 21.75			87 40 22	
		179 59 59.75	0.21	—0.26		
	The BASE of VERIFICATION between High Nook and Ruckinge . . .				. . . .	28532.92
	Lydd from { Ruckinge				. . . .	41533.89
	Lydd from { High Nook				. . . .	31362.58

N°

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spherical exceſs.	Diff. or error.	Angles corrected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
XXIV.	Allington Knoll .	91 27 20			91 27 19.5	
	Ruckinge .	54 19 17			54 19 18.5	
	High Nook .	34 13 21			34 13 22	
		179 59 58	0.69	−2.09		
	Allington Knoll from				High Nook	23184.93
					Ruckinge	16052.44

Hence, in the quadrilateral formed by *High Nook*, *Ruckinge*, *Lydd*, and *Allington Knoll*, making uſe of the two obtuſe angles, as contained within their now reſpective known ſides, we have for the mean diſtance of the points of the acute angles, at Lydd and Allington Knoll, repreſented by a dotted line in the plan of the triangles, 47849.27 feet. This diſtance agrees accurately with the length of the ſame ſide in the XXth triangle, as given by the baſe meaſured on Hounſlow Heath. Here however it is to be remarked, that, in order to produce this agreement, the angle at Hollingborn Hill, between Allington Knoll and Fairlight Down, has been made 48° 56′ 28″ inſtead of 48° 56′ 31″¼, being a difference of 3″¼+, which, according to obſervation, it ſhould have been. Had not this reduction been made, the diſtance between Allington Knoll and Fairlight Down, being one of the ſides of the XXIId triangle, would have been 106924 feet, that is to ſay, 1¼ foot longer. Now, ſince this ſide, compared with the baſe of verification, bears nearly the proportion of four to one, it follows, that the real difference between the meaſured length of that baſe, and its computed length deduced from that on Hounſlow Heath, ſeventy miles to the weſtward, or of

either baſe with reſpect to its oppoſite one, amounts only to about 4½ inches.

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spherical exceſs.	Diff. or error.	Angles corrected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
XXV.	Folkſtone Turnpike	24 17 6.25			24 17 6.25	
	Allington Knoll	76 1 54			76 1 53.25	
	High Nook . .	79 41 0.75			79 41 0.5	
		180 0 1	0.29	+0.71		
	Folkſtone Turnpike from Allington Knoll				. . . .	55461.7
	Folkſtone Turnpike from High Nook .				. . . .	54706.0
XXVI.	Folkſtone Turnpike	. . . .			32 6 56.89	
	Allington Knoll	109 50 40			109 50 39.35	
	Lydd . .	38 2 24			38 2 23.76	
			0.59			
	Folkſtone Turnpike from Lydd				. . . .	84659.88
XXVII.	Padleſworth .	108 9 34.5			108 9 34.5	
	High Nook .	. . . .			14 48 25.5	
	Folkſtone Turnpike	57 2 0			57 2 0	
			0.16			
	Padleſworth from High Nook .				. . . .	48303.7
	Padleſworth from Folkſtone Turnpike				. . . .	14713.82
XXVIII.	Padleſworth .	105 29 40.5			105 29 40	
	Lydd .	9 38 29			9 38 29.36	
	Folkſtone Turnpike	. . . .			64 51 50.64	
			0.27			
	Padleſworth from Lydd . .				. . . .	79533.34
	Padleſworth from Folkſtone Turnpike				. . . .	14713.82
XXIX.	Padleſworth .	12 16 3			12 16 2.65	
	Lydd .	154 5 54.75			154 5 54.4	
	Fairlight Down	. . . .			13 38 2.95	
			0.59			
	Padleſworth from Fairlight Down				. . . .	186113.0

N°

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spherical exceſs.	Diff. or error.	Angles corrected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
XXX.	Swingfield	48 38 15			48 38 15	
	Padleſworth	70 54 5.5			70 54 5.5	
	Folkſtone Turnpike	60 27 39.5			60 27 39.5	
		180 0 0	0.06	−0.06		
	Swingfield from Padleſworth				. . . .	17056.06
	Swingfield from Folkſtone Turnpike				. . . .	18525.15
XXXI.	Dover Caſtle, North Turret					
	Swingfield	34 39 26.5			34 39 26.5	
	Folkſtone	75 36 40			75 36 40	
		69 43 53.5			69 43 53.5	
		180 0 0	0.13	−0.13		
	Dover Caſtle from Swingfield				. . . .	30559.32
	Dover Caſtle from Folkſtone Turnpike				. . . .	31554.58

Hence, in the quadrilateral formed by *Folkſtone Turnpike*, *Swingfield*, *Padleſworth*, and the *North Turret of the Keep of Dover Caſtle*, making uſe of the two obtuſe angles, as contained within their reſpective known ſides, we have for the mean diſtance of the points of the acute angles at Padleſworth and Dover Caſtle 42561.18 feet; and hence, in the triangle, *Dover*, *Folkſtone Turnpike*, *Padleſworth*, we have the acute angle at Dover 15° 18′ 44″.4, and that at Padleſworth 34° 29′ 42″.4, as were repeatedly obſerved.

XXXII.	Dover Caſtle	. . . .			21 37 55.42	
	Padleſworth	152 15 25.5			152 15 25.15	
	Fairlight Down	. . . .			6 6 39.43	
			0.69			
	Dover Caſtle from Fairlight Down				. . . .	186113.0

N° of triangles.	Names of the ſtations.	Obſerved angles.	Spherical exceſs.	Diff. or error.	Angles corrected for calculation.	Diſtances.
		° ′ ″	″	″	° ′ ″	Feet.
XXXIII.	Dover Caſtle .	. . . .			87 30 29.58	
	Fairlight Down	. . . .			43 19 58.52	
	Montlambert .	. . . .			49 9 31.9	
			7.4			
	Montlambert from	Dover Caſtle .			. . . .	168821.07
		Fairlight Down			. . . .	245777.5
XXXIV.	Fairlight Down .	. . . .			25 33 55.02	
	Dover Caſtle .	. . . .			110 55 29.83	
	Blancnez .	. . . .			43 30 35.15	
			4.78			
	Blancnez from	Fairlight Down			. . . .	252469.9
		Dover Caſtle .			. . . .	116655.93
XXXV.	Dover Caſtle .	23 25 0.25			23 25 0.25	
	Montlambert .	. . . .			36 53 18.11	
	Blancnez .	. . . .			119 41 41.64	
			1.84			
	Blancnez from Montlambert				. . . .	77235.0

In this laſt triangle, the angle at Blancnez, as determined with great care from a mean of many obſervations, by the French Academicians, was found to be 119° 41′ 28″.9, that is to ſay, 12″.7 leſs than what reſults from our obſervations acroſs the Channel. This difference, which is the *maximum* of the error between us in the joint operation, being ſmall, and of no real importance one way or other, with regard to the main point in diſcuſſion, ſince it only varies the diſtance between Blancnez and Montlambert two or three feet, and the longeſt ſides of the triangles, which connect the two Coaſts, by eight or nine; it has therefore been judged beſt not

to

to make any alteration whatever on account of that difference (except as will be mentioned underneath), but to proceed with our own ſcale of diſtances for fixing the relative ſituation of Dunkirk; making uſe, nevertheleſs, in the firſt ſeven following triangles, from the XXXVIth to the XLIId inclusive, of the angles as ultimately ſettled by the late French operations, which Comte DE CASSINI has been ſo obliging as to ſupply us with for that purpoſe. The angles of the XLIIId and XLIVth triangles are taken from M. CASSINI DE THURY's Book *(La Méridienne verifiée)*; and thoſe of the XLVth triangle reſult from the combined operation.

In conformity with the exception above alluded to, we have, in the XXXVIth triangle, added 3″ to our angle obſerved at Dover between Blancnez and the *flèche* of the ſpire of Notre Dame at Calais; that is to ſay, inſtead of 12° 46′ 39″ it has been made 12° 46′ 42″, on a ſuppoſition, that the ſpire may overhang ſo much from the perpendicular towards Blancnez: becauſe the ſpace between the poſition of the white light on the gallery, and the axis of the ſpire, being carefully meaſured by Dr. BLAGDEN, correſponds to an angle of 9″, whereas the obſervation gave only a difference of 6″.

N° of triangles.	Names of the ſtations.	Angles corrected for calculation.	Diſtances.
		° ′ ″	Feet.
XXXVI.	N.D. at Calais	47 27 6	
	Blancnez ſignal	119 46 12	
	Dover Caſtle . . 12° 46′ 39″	12 46 42	
	N.D. at Calais from Dover Caſtle		137449.9
	N.D. at Calais from Blancnez ſignal		35023.3
	Exceſs above 180° = 0″,84.		

N°

N° of triangles.	Names of the ſtations.	Angles corrected for calculation.	Diſtances.
		° ′ ″	Feet.
XXXVII.	Fiennes ſignal.	94 26 27.5	
	Blancnez ſignal	51 18 27.3	
	Montlambert ſignal	34 15 5.2	
	Fiennes ſignal from Blancnez ſignal	. . . .	43600.8
	Fiennes ſignal from Montlambert ſignal	. . . .	60464.4
XXXVIII.	N.D. at Calais	64 21 43.1	
	Fiennes	46 24 25.2	
	Blancnez	69 13 51.7	
	N.D. at Calais from Fiennes	. . . .	45219.6
XXXIX.	Watten	27 37 14.8	
	N.D. at Calais	66 30 36.2	
	Fiennes	85 52 9	
	Watten from Fiennes	. . . .	89453.5
	Watten from N.D. at Calais	. . . .	97283.0
XL.	Dunkirk	51 39 12	
	Watten	85 57 46.5	
	N.D. at Calais	42 23 1.5	
	Dunkirk from N.D. at Calais	. . . .	123734.8
	Dunkirk from Watten	. . . .	83616.2
XLI.	Mont-Caſſel	63 24 50	
	Dunkirk	42 7 12	
	Watten	74 27 58	
	Mont-Caſſel from Watten	. . . .	62711.1
	Mont-Caſſel from Dunkirk	. . . .	90087.5
XLII.	Hondſcôte	93 31 34.1	
	Dunkirk	51 7 4.5	
	Mont-Caſſel	35 21 21.4	
	Hondſcôte from Mont-Caſſel	. . . .	70260.7
	Hondſcôte from Dunkirk	. . . .	52228.4
XLIII.	Dunes, ſignal at the eaſt end of the baſe	81 57 30	
	Dunkirk	48 43 23	
	Hondſcôte	49 19 7	
	Signal on the Dunes from Hondſcôte	. . . .	39641.0
	Signal on the Dunes from Dunkirk	. . . .	40000.5
XLIV.	Dunes ſignal	5 19 47	
	Fort Revers, weſt end of the baſe	90 17 29	
	Dunkirk	84 22 44	
	Dunkirk from Signal on the Dunes	. . . .	40000.5
	Dunkirk from Fort Revers	. . . .	3715.6
	Fort Revers from the Dunes, the meaſured baſe	. . . .	39808.7
	But this baſe by meaſurement was	. . . .	39801.7
	The difference is	. . . .	7.0

In

In order to complete the triangular connection between Greenwich and Paris, there remains yet one triangle more (the XLVth) to be given, whereby we ſhall be enabled to connect the point M near Dunkirk with Dover. For this purpoſe it is neceſſary to make ſome remarks on the Dunkirk baſe; and alſo to ſhew, from the French operations, how the point M is ſituated with reſpect to Paris, Dunkirk, and Calais.

M. CASSINI DE THURY, in his book already quoted (p. 23. and 54.) has informed us of the manner in which this baſe on the Strand near Dunkirk was meaſured; and that its mean length amounted to 6224.36 toiſes, which are equal to 39801.7 Engliſh feet. Thus it appears, that there is a difference as above ſtated of ſeven feet in defect, between the meaſured and computed length of the laſt ſide of a combined ſeries of 44 Britiſh and French triangles, depending on a baſe meaſured on Hounſlow Heath, and verified by another meaſured in Romney Marſh. But a ſeries of 24 French triangles, founded on a baſe meaſured near Paris, and corrected by another executed near Amiens, gives for the length of the ſame baſe near Dunkirk 39809.94 Engliſh feet, and conſequently only an exceſs of 15 inches with regard to our reſult. This very near agreement in the determination of the ſame length by two different ſerieſes of triangles, whoſe extremities are ſituated at ſo great a diſtance from each other, ſufficiently proves the excellency of trigonometrical meaſurement, and ſhews to what a wonderful degree of accuracy operations of this ſort may be brought when fine inſtruments are made uſe of, and great care beſtowed in the application of them. Doubtleſs ſmall errors may have ariſen in the progreſs of the work, unſeen on both ſides; but theſe falling ſometimes one way and ſometimes the other, they ſeem ſo far to have compenſated for, or deſtroyed each other,

that their effects have almoſt wholly diſappeared. With regard to the deficiency of ſeven feet found between the actual meaſurement with the deal rods, on the Strand near Dunkirk, and the trigonometrical reſult, it is neceſſary to call to remembrance what would have happened, if the baſe on Hounſlow Heath had been meaſured with our deal rods, when in their greateſt ſtate of expanſion from the moiſture they had imbibed. In the volume of Philoſophical Tranſactions for 1785, p. 438. it has been ſhewn, that our baſe would thereby have been rendered more than ſeven feet and a half too ſhort. Now, although the French rods were covered with ſeveral coats of oil-paint to prevent their imbibing the ſalt water, which we are told reſted on the Strand at particular places ſix inches deep; yet it is preſumable, that it would be impoſſible to prevent it from entering by the extremities at the junction of the ferrules, and extending along the fibres, underneath the paint. Hence, in all likelihood, the intended remedy would prove worſe than the diſeaſe: for the paint might prevent the rods from drying ſo ſoon as they otherwiſe would have done, and thereby the meaſurement would be given ſtill ſhorter than if no paint had been applied. Whether this ſuppoſition may be thought to be well founded or not, is left to the determination of thoſe who are converſant in matters of this ſort. But the curious fact, one way or other, might be aſcertained by means of ſuch a ſteel chain as ours, in the ſpace of one or two days at moſt. For, ſuppoſing the extremities of the baſe between *Fort Revers* and the *Dunes* to be accurately known, and the *allignement* traced out on the Strand only with camp colours, placed at reaſonable diſtances from each other, and a moveable cord, by the ſimple application of the chain on the common ſurface, without any extraordinary apparatus whatever, forwards

wards and again backwards, the diſtance might certainly be determined within a foot of the truth. And hence the importance is obvious of having at all times ſo accurate and eaſy a mode of meaſurement.

On due conſideration of all theſe circumſtances, it will not be thought ſurpriſing, that in fixing the ſituation of Dunkirk and the point M near it, where the meridian of the Royal Obſervatory at Paris interſects a line drawn from thence to N.D. at Calais, the *Dunkirk baſe*, with the corrections depending upon it, are here rejected; and that the ſcale of diſtances furniſhed by the Britiſh triangles is adhered to, as not differing ſenſibly from the mean reſult given by the other two French meaſurements.

From M. DE CASSINI's Book, *La Méridienne Vérifiée*, p. 51. 53. and 56. it appears, that Dunkirk (rejecting the corrections formerly alluded to) is north from the Royal Obſervatory at Paris 125522.2 toiſes, which are equal to 133775.3 fathoms. And from p. 51. and 57. it further appears that, by the mean of two ſuites of triangles, Dunkirk is eaſt from the meridian of Paris 1420.41 toiſes, which are equal to 1513.8 fathoms. Again, at p. 276. of the ſame Book, Dunkirk is ſaid to be north 125517 toiſes, and eaſt 1430 toiſes, which are reſpectively equal to 133769.7 and 1524.2 fathoms. And, laſtly, at p. 36, of the *Déſcription Géometrique de la France*, of the ſame Author, publiſhed in 1783, and which being the lateſt ſhould of courſe be the moſt correct work, Dunkirk is made north from the Royal Obſervatory 125495 toiſes, and eaſt from its meridian only 1416 toiſes, which are reſpectively equal to 133746.3 and 1509.1 fathoms. Now, without pretending here to enter into the inveſtigation of the various corrections + and − which have been applied to the angles of the tri-

angles, to bring out these different results, we shall abide by the first, that being immediately produced by the mean of the observations without any arbitrary correction whatever; and being, with regard to easting, nearly a mean between the two extremes; and since we have it in our power to settle with great precision the longitude of Dunkirk, and likewise the point M, with regard to Greenwich, we shall then be enabled to determine the difference of longitude between the two Royal Observatories within a mere trifle of the truth.

By Comte de Cassini's triangles, executed in the autumn of 1787, and communicated in January 1789, it appears, that Hondscôte is south-eastward from the meridian of Dunkirk 67° 53′ 20″; which angle being subtracted from the total angle between Hondscôte and Calais 144° 53′ 28″.5, being the sum of the three angles at Dunkirk in the XLth, XLIst, and XLIId triangles, there remains 77° 0′ 8″.5 for the angle that Calais is south-westward from the meridian of Dunkirk. And this last angle being again subtracted from 180°, we have 102° 59′ 51″.5 for the angle between the same meridian produced northward, and a line drawn from Dunkirk through M to Calais.

Now the two distances 133775.3 and 1513.8 fathoms being severally reduced in the proportion of 39809.94 to 39808.7 the two lengths assignable to the base on the Strand near Dunkirk, as formerly established, we have 133771.1 fathoms for the distance in British measures of the parallel of Dunkirk from that of the Royal Observatory at Paris; and 1513.75 fathoms for the distance of Dunkirk eastward from its meridian. Again, making use of the angle 77° 0′ 8″.5, and its complement to 90° = 12° 59′ 51″.5, we have 1553.56 fathoms for the direct distance between Dunkirk and the point M;

alſo 358.6 fathoms for the ſpace that M is ſouthward from Dunkirk. But the diſtance of Dunkirk from the Royal Obſervatory at Paris, given in the 276th page of M. CASSINI DE THURY's Book, when reduced in the proportion of the two baſes becomes 133765.7 fathoms, and taking a mean between this number and that formerly found 133771.1 fathoms, we have for the mean diſtance of Dunkirk from the Obſervatory 133768.4 fathoms, from which ſubtracting 358.6 fathoms, the mean ſouthing of M from Dunkirk, there will ultimately remain 133409.8 fathoms for the diſtance between M and the Royal Obſervatory at Paris, meaſured on the meridian.

Now, ſince in the XLth triangle we have the diſtance of N.D. at Calais from Dunkirk 123734.8 feet, if from this number we ſubtract 1553.56 fathoms = 9321.36 feet, there will remain 114413.4 for the diſtance of the point M from Calais. Thus, with the ſupplemental angle to 360° at Calais, *viz.* 139° 17′ 33″.2, contained within its now known ſides, we are finally enabled to complete the XLVth triangle, and thereby to determine the ſituation of M with regard to Dover.

XLV.

	°	′	″	
N.D. at Calais	139	17	33.2	The ſpherical exceſs above 180° = 2″.42.
Dover Caſtle	18	24	37.3	
M near Dunkirk	22	17	49.5	

		Feet.
N.D. at Calais from	Dover Caſtle	137449.9
	M near Dunkirk	114413.4
Dover Caſtle from M		236273.7
Alſo, Dover is from Dunkirk		243291.3

ART. III. *Reſult of the trigonometrical operation, in as far as relates to the geodetical ſituation of the different ſtations, with regard to the Royal Obſervatory at Greenwich.*

Having, by the preceding calculations of the lengths of the ſides and meaſures of the angles of a continued ſeries of

forty-five triangles, determined the relative ſituation of every ſtation with regard to thoſe neareſt adjacent to it, we are next to ſhew, from theſe *data*, and the angles which Norwood and Severndroog Caſtle make with the meridian of Greenwich Obſervatory, the ſituation of each ſtation with reſpect to that meridian, to its perpendicular, and alſo the direct or diagonal diſtance with the bearing from the Obſervatory itſelf. Theſe various determinations are contained in the ſix firſt columns towards the left hand of the annexed table of reſults, wherein the ſtations are likewiſe diſtinguiſhed into two ſets, as ſituated to the weſtward or eaſtward of Greenwich.

By means of a ſcaffold, perfectly ſimilar in principle to that formerly deſcribed, but more ſlight as being made for the temporary purpoſe only, the ſtand of the inſtrument was raiſed to the height of thirty-eight feet above the floor of the tranſit-room of the Obſervatory. At this elevation all the ſurrounding objects which we wiſhed to obſerve (St. Paul's excepted, which is hidden by the camera-turret of the great room) could be diſtinctly ſeen, and the angles between them and the ſouth meridian mark accurately meaſured. As that mark is but at a ſhort diſtance, namely, about 1500 feet from the tranſit, and conſequently $\frac{1}{10}$th of an inch correſponding to about a ſecond of an angle on the mark, it was therefore very neceſſary that the center of the inſtrument ſhould be brought with great preciſion over the center of the axis of the tranſit-teleſcope underneath. In this operation, and indeed in every other while at Greenwich, the Aſtronomer Royal gave us his beſt aſſiſtance. In the firſt place, the central point of the axis was determined by the interſection of diagonal lines drawn acroſs the ſquare part in the middle. On this ſquare part, when the teleſcope was in its horizontal poſition, a baſon of quickſilver was placed,

placed, having a ſmall croſs made of two thin bits of wood fitted to the inſide of the baſon, and lying very near the ſurface of the quickſilver, in ſuch a manner as to make the center of the croſs co-incide with the interſection on the braſs underneath. A ſmall perſpective glaſs being then fixed in a moveable board under the center of the inſtrument, this was made to ſlide at right angles to itſelf in the direction of the meridian and that of the axis of the tranſit, until the center of the croſs coincided with the axis of viſion in looking downwards. The board being there faſtened, and the perſpective removed, the interſection of ſilk threads ſtretched acroſs the board, marked very accurately the point correſponding with the center of the tranſit, over which the center of our inſtrument was brought by the help of the plummet. The ſecond method was ſtill more direct. Dr. MASKELYNE had an object glaſs prepared for his tranſit teleſcope of a focus ſuited to the vertical height of the ſtand of our inſtrument above it. This glaſs being applied to the tranſit, and the aperture contracted by a piece of paſteboard with a circular hole in the middle, a very ſmall pin-hole being likewiſe made in the board at top, the ſame was gradually moved by directions from the obſerver below, looking through the teleſcope in its vertical poſition, until the pin-hole nicely co-incided with the axis of viſion. The inſtrument was then brought as before, by the help of the plummet, exactly over the pin-hole. In this manner, which was that adhered to, no doubt remained of more than about $\frac{1}{100}$th part of an inch, with reſpect to the center of the inſtrument being in the interſection of two vertical planes paſſing through the axis of viſion and that of motion of the tranſit underneath. After having remained a week, the co-incidence of the pin-

hole with the axis of vision of the telescope was tried, and found to have suffered no alteration.

In the VIIIth triangle, the angle at Greenwich, between Severndroog Castle on Shooter's Hill and the station on Norwood heights, hath been shewn to be 111° 56′ 50″. By several observations on different parts of the circle, Norwood station was found to be westward from the meridian 38° 7′ 16″, which of course leaves for the angle that Severndroog Castle is eastward from it 73° 49′ 34″; and either of these two angles is supposed to be within a very small fraction of a second of the truth.

Now, with the sides and angles of the series of triangles already known, and the angle 38° 7′ 16″ now given, which Norwood makes with the meridian of Greenwich towards the west, it will be sufficiently obvious to those who are in the least acquainted with plane trigonometry, that the distances of that or of any other station of the series, from the meridian of Greenwich and from its perpendicular, are easily obtained. Nevertheless, that those who are but little conversant with matters of this sort may themselves be able to examine the computations whereby the columns towards the left-hand of the annexed table have been supplied, we shall give one example, which will serve for the whole.

Suppose (Plate X. fig. 1.) GM to represent the meridian of the transit-room at Greenwich; GW the perpendicular to that meridian produced indefinitely towards the west; N the station at Norwood, and H that at Hundred Acres, whose distances are required, that is to say, westward from the meridian, and southward from the perpendicular: then through the stations N and H, let dotted lines be drawn parallel to the meridian and perpendicular respectively, whereby four parallelograms will

will be formed. In the firſt, or that which is neareſt to Greenwich, having GN in the VIIIth triangle given = 31274.48 feet, and the angle NG*m* = 38° 7′ 16″, with its complement NG*p* = 51° 52′ 44″, it follows, that N*m* repreſenting the diſtance of Norwood weſtward from the meridian is = 19306.54 feet; and N*p* repreſenting its diſtance ſouthward from the perpendicular is = 24603.86 feet. Again, by attending to, and ſumming up the angles round the point N, we ſhall find the angle GNH = 175° 44′ 36″.82, which wanting 4° 15′ 23″.18 of 180°, ſhews that the direction of the ſide NH inclines ſo much more to the weſtward than the angle NG*m*. Wherefore NG*m* = 38° 7′ 16″ + 4° 15′ 23″.18 = 42° 22′ 39″.18 = HN*s*, is the angle which the line NH makes with N*s*, a parallel to the meridian of Greenwich drawn through the point N. Now, in the ſupplemental parallelogram, having the diagonal NH = 35648.21 given in the VIIth triangle, and the angle HN*s* = 42° 22′ 39″.18, alſo its complement = 47° 37′ 20″.82, making uſe of NH as radius, and theſe two laſt angles reſpectively, we have *s*H = 24027.36 feet for the ſpace that H is more weſtward than N; and *w*H = 26334.04 feet, that H is more ſouthward than N. Hence *m*N + *s*H = 43333.9 feet is the ſpace that H is to the weſtward of Greenwich; and *p*N + *w*H = 50937.9 feet is the ſpace that H is ſouthward from the perpendicular to the meridian of Greenwich. Laſtly, with theſe two given ſides, and the contained angle 90°, we find the angle MGH = 40° 23′ 18″.54, that Hundred Acres is ſouth-weſtward from the meridian of Greenwich; whence the direct or diagonal diſtance GH = 66876.73 feet. Now, by referring to the table of reſults, for the two firſt ſtations weſtward from Greenwich, the numbers brought out in this example will be found in the left-hand columns under their reſpective heads, and ſo it would be with the

the reſt. In another place we ſhall have occaſion to point out, how the columns towards the right-hand of the ſaid table have been filled up.

SECTION FIFTH.

On the difference between horizontal angles on a ſphere and ſpheroid. Plate X.

IN the Paper of 1787, various computations were given concerning the figure and dimenſions of the earth, founded chiefly on the actual meaſurement of different arcs of the meridian in different latitudes, ſome of them very remote from each other. From the alternate compariſon of theſe reſults it appeared, that the figure aſſigned to the earth by M. BOUGUER in his ſecond ſpheroid agreed better with theſe meaſured portions of the curve, as ſo many *data*, than any of the other *hypotheſes*. Hence it naturally occurred, that the trigonometrical operation which we were then about to commence might probably throw ſome further light on this intricate ſubject, which, for a great length of time, has engaged a conſiderable ſhare of the attention of the ſcientific world. In the conſideration of this matter, a new and curious point, not formerly attended to, and immediately connected with our operation, preſented itſelf for inveſtigation, viz. *ſuppoſing the earth to be a ſpheroid, ſuch as M.* BOUGUER's, *conſiderably flattened at the poles, what might be the difference between horizontal angles obſerved with a fine inſtrument on that ſpheroid, and on a ſphere?*

The

The following ſolution * of that important problem, being the only unexceptionable one that I have received, is here given in the author's (Mr. DALBY's) own words.

Let CE and CP (Plate X. fig. 2.) repreſent the equatorial and polar ſemi-diameters of the earth, conſidered as a ſpheroid flattened at the poles; PE and PN two meridians; *pe* and *pn* two correſponding ones (that is, in the ſame planes) on a ſphere, having the ſame center C. Let the points *a*, *b*, A, B, on the ſphere and ſpheroid have the ſame latitudes reſpectively. Draw the radii *a*C, *b*C, and the verticals AG, BW.

Then, becauſe the angles AON, BDE, in the ſpheroid, are always equal to the latitudes of the points A, B, theſe angles are therefore reſpectively equal to the angles *a*C*n*, *b*C*e*, in the ſphere, and conſequently the verticals AG, BW, are parallel to the radii *a*C, *b*C.

Let the latitude of B or *b* be greater than that of A or *a*; and let it be required to make the horizontal angle PA*r* on the ſpheroid equal to the angle *pab*, or what the horizontal angle would be on the ſphere.

Becauſe the angle *pab* is meaſured by the inclination of the planes, *a*C*b*, *a*C*p*, and AG is the common interſection of all the planes of the vertical circles at A, and is parallel to *a*C, and in the ſame plane; therefore, when the horizontal angle PA*r* is equal to the angle *pab*, the planes GA*r*, CA*b*, muſt be parallel to each other; and conſequently G*r*, the line where the plane GA*r* interſects the plane of the meridian EP, is pa-

* From my correſpondence by letter, and otherwiſe, with Dr. MASKELYNE, I had reaſon to hope, that he would have favoured me with ſome communication on this ſubject. No doubt, he has been prevented by other buſineſs; but he will probably give his method of ſolving ſpheroidical triangles to the Royal Society on ſome future occaſion.

rallel to C*b* in the ſphere, or WB in the ſpheroid. Hence, if from G, the point where the vertical AG meets the axis, we draw G*r* parallel to the vertical BW, it will give the point *r* in the meridian EP, making the horizontal angle PA*r* equal to the angle *pab*, or what the horizontal angle would be on the ſphere.

In like manner, if the angle PB*v* is to be made equal to the angle *pba*, W*v* muſt be drawn parallel to GA, and the plane *v*WB will be parallel to the plane AG*r*; and therefore the angles of the ſpheroidical triangles PA*r*, P*v*B, as meaſured by the inclination of the planes, are equal to each other reſpectively, and equal to the ſpherical angles of the triangle *pab*.

From hence it follows, that if A be the place of an inſtrument which meaſures horizontal angles in the meridian NP on a ſpheroid, and BT a flag-ſtaff ſet perpendicular to the ſurface of the earth on another meridian EP, the obſerved horizontal angle PAB, between the meridian PA and the flag-ſtaff BT, will be greater than it would be on a ſphere (the latitudes and longitudes being the ſame in both) as long as the latitude of the flag-ſtaff is greater than that of the inſtrument, the exceſs being the angle BA*r*; but if the latitude of the inſtrument is the greateſt, as ſuppoſe it was at B, and the flag-ſtaff at A, then the obſerved angle PBA will be leſs than it would be on the ſphere, the defect being the angle AB*v*, which, becauſe the planes W*v*B, GA*r*, are parallel, will be the ſame as the exceſs on the other ſide.

If the latitudes of the points A and B are the ſame, the planes W*v*B, GA*r*, will co-incide, or the verticals will meet in the ſame point in the axis, and therefore the obſerved angles will be equal to each other, and the ſame as they would be if obſerved on a ſphere.

Becauſe

Becauſe AG, *v*W, BW, *r*G, are parallel to *a*C, *b*C, the angles *v*WB, AG*r*, will be equal to the angle *a*C*b*, or arc *ab*, therefore the arcs *v*B, A*r*, will each be equal to the arc *ab*; that is, they are arcs of great circles of the ſame value, intercepted between the meridians PN, PE, at B and A.

Draw GR perpendicular to the vertical BW; then, becauſe BW and *r*G are parallel, it will alſo be perpendicular to *r*G; and becauſe the axis PW is the common interſection of the planes of all the meridians, and BW, *r*W, are in the plane of the meridian PB, therefore GR is in that plane; and becauſe the angle WB*r*, made by the vertical and meridian, and the angle GRB, are right ones, therefore GR is equal to the arc B*r* nearly, and conſequently is nearly equal to what ſubtends the difference of the horizontal angles on the ſphere and ſpheroid.

And if GS be perpendicular to the vertical GA, it will be equal to the arc A*v* nearly, and therefore GR, GS, or the arcs B*r*, A*v*, will be as the coſines of the latitudes of B and A.

Draw AK the tangent to the meridian at A, to meet the axis CP produced; alſo draw AH perpendicular to the vertical AG, to meet G*r* produced; through H draw KHT, and join AT. Then, becauſe the points K, H, are in the plane of the horizon of A, the line KHT will be in that plane; and becauſe *r*H and BT are in the plane of the meridian BP, therefore HT is alſo in the ſame plane, and is what ſubtends the angle TAH, the true difference of the horizontal angles, which, when the ſpheroid is given, may be determined as follows.

From the nature of the ſpheroid, find the length of the vertical AG; alſo the points G and W, where the verticals meet the axis: then, becauſe the angle AKG is equal to the latitude of A, and AGH is its complement, GK and AK will be given.

Let *a* and *b* on the ſphere have the ſame latitudes and difference of longitude as A and B on the ſpheroid, and find the angles *pab*, *pba*, and the arc *ab*, or angle *aCb*; then becauſe AG is given, and the angle AGH equal to the angle *aCb*, AH will be given; with AH and AK, and the included angle HAK (equal to the ſpherical angle *bap*) find the angle AHK, and alſo KH; then, becauſe the triangles KHG, KTW, are in the ſame plane (that of the meridian BP) and GH is parallel to WT, theſe triangles will be ſimilar. Hence GK : HK :: WG : TH; now HA, HT, and the included angle AHT (the complement of AHK) being given, the angle TAH, the difference of the horizontal angles, will be given.

Example. Let the ſpheroid be that of M. Bouguer; and let the latitude of A be 49° 40′, of B 50°, and their difference of longitude 0° 30′.

From the nature of the ſpheroid, the radii of curvature of the meridian at the equator and the pole, will be 3465507 and 3524069 fathoms nearly; their difference is 58562 fathoms, the length of the evolute of the meridian; and the vertical $AG = 3465507 + \frac{3}{7} \times 58562 + \frac{1}{5} \times 58562 \times \overline{\text{fine } 49° 40'}^4 + \frac{4}{7} \times 58562 \times \overline{\text{fine } 49° 40'}^2 = 3509769.5$ fathoms; alſo $OG = \frac{3}{7} \times 58562 + \frac{4}{7} \times 58562 \times \overline{\text{fine } 49° 40'}^2 = 40307.66$ fathoms; and $DW = \frac{3}{7} \times 58562 + \frac{4}{7} \times 58562 \times \overline{\text{fine } 50°}^2 = 40397.23$ fathoms. Now, the angles GOC, WDC, being = the latitudes of A and B, we get CG = 30726.16, and CW = 30946.08 fathoms, their difference being 219.92 fathoms = GW.

The ſides *pa*, *pb*, being equal to 40° 20′ and 40° reſpectively, and the included angle = 30′, will give the angle *pab* = 43° 51′.48″.2, the angle *pba* = 135° 45′ 16″.2, and *ab*, or the angle *aCb*, = 27′ 49″.7.

Now

Now, by proceeding as directed above, we get GK = 4604232.9, AK = 2980006.3, and AH = 28412.2 fathoms. Hence the angle AHK = 135° 45′ 20″.08, and KH = 2979745.4 fathoms; whence HT = 141.37 fathoms. This, with AH, and the included angle AHT = 44° 14′ 39″.92 (the complement of AHK) give the angle TAH = 11′ 58″.9, the difference between the horizontal angles on the ſphere and ſpheroid.

Hence the obſerved angles at A and B would be 43° 51′ 48″.2 + 11′ 58″.9 = 44° 3′ 47″.1, and 135 45′ 16″.2 − 11′ 58″.9 = 135° 33′ 17″.3.

If the figure is an ellipſoid having the ſame *axes*, the angle TAH will be found = 8′ 4″.4.

It may be remarked, that the angle TAH, or the horizontal angle TAK, diminiſhes or augments as the point obſerved in TB is elevated or depreſſed; this variation is however too ſmall to be worth attending to in practice, as may be ſhewn in the following manner.

Let the ſpheroid be M. BOUGUER's (becauſe the difference will be greater than on an ellipſoid); and let the points A, B, fig. 3. have the ſame latitudes and difference of longitude as above; alſo, let BT be the flag-ſtaff, and through B draw GB*n*.

Now, if we ſuppoſe B to be in the horizontal line nearly, the horizontal angle at A, taken between the north part of the meridian AP and the flag-ſtaff at B, will be the angle BAP, the teleſcope in this caſe being pointed to B, and the vertical plane which it would then move in is the plane *n*BGA; but if the teleſcope is directed to ſome point T in the flag-ſtaff above B, the angle TAP in this caſe will evidently be leſs than it was in the former by the angle *n*AT nearly; and conſequently it diminiſhes as the obſerved point T is elevated; and it is alſo

evident, that it will be augmented as the point obſerved is below B.

The latitudes of A and B being 49° 40′ and 50°, and their difference of longitude 30′, BG will be nearly equal to AG, or 3509769 fathoms, and GR being equal to 141.36 fathoms, and the angle GRB a right one, we have BG (3509769) : rad. :: GR (141.36) : ſin. 8″, the angle GBW, or TB*n*. Now, ſuppoſing the point T to be a mile above the ſurface, this with the angle *n*BT = 8″, will give T*n* equal to about three inches; but T*n* is in the plane of the meridian PBE, and conſequently would be ſeen obliquely, if viewed from A, becauſe the angle ABT is about 135°, and therefore T*n* muſt ſubtend a very ſmall angle at the diſtance of 33¼ miles, which is nearly the diſtance between A and B.

From the determination of the horizontal angles that would be obſerved at A and B (fig. 2.) on the ſpheroid, if AP, BP, the co-latitudes of A and B are known, and the angles ABP, BAP, are given by obſervation, it follows, that the greater of theſe obſerved angles muſt be augmented, and the leſſer diminiſhed, by the ſame quantity of a degree, till the ſum and difference give the oppoſite ſides AP, BP, accurately by ſpherical computation, and then the third angle, or difference of longitude, will be given: for the obſerved angles at B and A being reſpectively 135° 33′ 17″.3 and 44° 3′ 47″.1, we have ſine 135° 33′ 17.″3 + 11′ 58″.9 : ſine AP :: ſine 44° 3′ 47″.1 − 11′ 58″.9 : ſine BP accurately; but taking the angles that would be obſerved, ſine 135° 33′ 17″.3 : ſine AP :: ſine 44° 3′ 47″.1 : ſine of an arc greater than BP; and ſine 44° 3′ 47″.1 : ſine BP :: ſine 135° 33′ 17″.3 : ſine of an arc leſs than AP; and this will ſhew if the obſerved angles are conſiſtent, as angles that

onght to be found by obſervation on a ſpheroid flattened at the poles.

Becauſe the ſum of the obſerved angles at A and B on the ſpheroid are equal to the ſum that would be obſerved on a ſphere, the latitudes and difference of longitude being the ſame on both, and the differences equal, therefore the ſum for computation is the ſame for both, and the quantity of each for computation on the ſpheroid may be found from the following

Theorem.

In any ſpherical triangle BPA (fig. 4.) if two of the ſides PB, PA, and the ſum of the oppoſite angles, PBA + PAB, are given, it will be,

As the tangent of half the ſum of the ſides,
Is to the tangent of half their difference;
So is the tangent of half the ſum of the angles,
To the tangent of half their difference.

In the ſpherical triangle *abp* (fig. 2.), as ſine *bap* : ſine *bp* :: ſine *abp* : ſine *ap*; that is, on the ſpheroid, ſine B*v*P : ſine BP :: ſine *v*BP : ſine AP. Now, the arc B*v* being = the arc *ba*, conſidered as an arc of a great circle, it follows, that in the ſpheroidical triangle *v*BP, if *v*B, BP, and the included angle *v*PB are given, the other angles at P and *v* may be found by ſpherical computation, but not the third ſide *v*P. Suppoſe BP, B*v*, are given, and the included angle *v*BP a right one; then rad. : ſine BP :: cotang. B*v* : cotang. angle BP*v*; therefore, if the latitude of the point B, and the angle BW*v*, or the quantity of the arc B*v*, as an arc of a great circle perpendicular to the meridian at that point, are given on a ſpheroid, the difference of longitude may be found by ſpherical computation, but not the latitude of the point *v*.

But

But if the ſpheroid is known, the latitude of a given point (v) in a great circle perpendicular to the meridian, may be found nearly from what has been delivered above. Thus, as rad. : coſine BP :: coſine Bv : coſine of an arc (PA) leſs than Pv, the co-latitude of v. Now, with the latitude (ſuppoſe of the point A) thus found, and the given latitude of B, find GS (fig. 2.) which will be very nearly equal to the arc Av, and the value of this, as an arc of the meridian, being added to PA, will give Pv, the co-latitude of v.

SECTION SIXTH.

Manner of determining the latitudes of the ſtations. Application of the pole-ſtar obſervations to computations on different ſpheres, and alſo on M. BOUGUER's *ſpheroid, for the determination of the differences of longitude. Ultimate reſult of the trigonometrical operation, whereby the difference of the meridians of the Royal Obſervatories of Greenwich and Paris is determined. Plate X.*

ARTICLE I. *Preamble, ſhewing the general principles adopted for ſettling the latitudes of the ſtations.*

In the Paper of 1787, ſo often quoted, and which was intended only as a ſketch of the mode then propoſed to be followed in conducting the recent trigonometrical operation, we had occaſion to ſhew, that the meaſured arc of the meridian between the point M near Dunkirk, and Perpignan ſituated at the

the bottom of the Pyrenean mountains, corresponding to an arc in the heavens of nearly $8\frac{1}{4}$ of latitude, differed but little from what should be its true length, supposing the earth to have the figure and dimensions assigned to it by M. Bouguer in his second spheroid. Here, however, it is become necessary to take notice of some mistakes * that, through inadvertency, were fallen into in the computed lengths of the arcs, which, although they affect in a certain degree the accuracy of the numbers brought into comparison, do not invalidate the general reasoning there advanced, and the only thing meant to be established, namely, that M. Bouguer's hypothesis agreed better with actual measurement on different parts of the surface of

* The mistakes adverted to in the text were of three kinds. First, an erroneous mode of summing up the lengths of the arcs from the lengths of the degrees, although these taken separately were very accurately computed: for instance, the 43d was taken as that extending from 42 to 43, whereas it should have been taken for the middle point, that is, from $42\frac{1}{2}$ to $43\frac{1}{2}$, and so on in regard to others. Hence the arcs are all made somewhat too long. The second was the omission of the value of $93\frac{1}{2}$ toises in estimating the length of the celestial arc between Greenwich and Perpignan, the sector with which the stars were observed having stood so much to the northward of the church of St. Jaumes, the point to which the triangular measurement corresponded. The third was fallen into from not knowing that the French observations of the stars had been corrected for the nutation of the earth's axis, in a Paper of M. de la Caille's, inserted in the Memoirs of the Academy of Sciences for the year 1758, whereby all the lengths of the celestial arcs were thereby in some degree changed from what had been assigned to them respectively in the Book, *La Méridienne vérifiée*, published in 1744. From the same Paper it further appears, that they rejected altogether their observations at Perpignan, as being probably affected by the attraction of the Pyrenées. With regard to that part of the Table of Comparison in the Paper of 1787, which is affected by these errors, the only thing that now can be done is to annex to this paper a corrected slip, which may be referred to occasionally, or cut off and pasted over the former.

the earth, than any of the others with which it was compared.

In proof of this, we need only for the preſent remark, what will be made fully to appear hereafter, that the diſtance between the parallels of Greenwich and Dunkirk, or Greenwich and M, being now added (by our trigonometrical operation) to the meaſured length of the meridian of France, the meaſured and computed ſections of the united meridian will be found to agree almoſt exactly at Paris; that the exceſs of the meaſurement is but of the value of 3″ or 4″ at Bourges; only of 6″ at Rodés; aud even as low down as Perpignan, comprehending in the whole an arc of the heavens of more than 8°¼, the exceſs is not greater than what would anſwer to between 16″ and 17″, the chief part of which is probably owing to the attraction of the plummet of the ſector by the Pyrenees. In the Paper of 1787, the effect had been aſſumed at a quantity equal to about 10″. But every thing on this head muſt be conſidered as merely matter of ſuppoſition, which cannot be determined one way or other until triangular meaſurements ſhall have been extended beyond the Pyrenean mountains into Spain, and correſponding obſervations of the ſtars made on both ſides with the ſame inſtrument, which ſhould be one of the beſt that could poſſibly be invented for the purpoſe. In the mean time, ſince the French have rejected their own obſervations at Perpignan, we ſhall avoid drawing any concluſions with regard to latitudes from the obſervations to the ſouthward, and confine ourſelves to thoſe immediately connected with our operation, made at the northern ſtations of the meridian.

In carrying on the trigonometrical operation, it never was propoſed that we ſhould attempt to determine the latitudes of the

the ſtations, by actual obſervations of the zenith diſtances of ſtars, which, with the very beſt inſtruments hitherto uſed for that purpoſe, could not have been done nearer than about 1″ of an angle in the heavens, anſwering in theſe parts to 101 feet on the ſurface of the earth. Even if we could have been ſupplied with a ſector ſo far ſurpaſſing the old ones (ſuch perhaps as Mr. RAMSDEN may hereafter invent) that would have given zenith diſtances to one-tenth part of a ſecond, or about ten feet on the ſurface of the earth, the application of it in our operation would have been mere loſs of time: for the Aſtronomer Royal having ſettled the latitude of Greenwich 51° 28′ 40″, to within leſs than half a ſecond of the truth; and the geodetical ſituation of each ſtation of our ſeries being determined ſo accurately with regard to that point, as to leave no where an uncertainty of more than one or two feet; we have thereby been able to determine the relative latitudes to a ſmall fraction of a ſecond. Here, however, it is to be underſtood, that we have adhered to M. BOUGUER's ſcale, as anſwering almoſt exactly in the narrow ſpace of 26′ 51″, or thereabout, of latitude between Greenwich and M, to which our operations have been confined.

That this mode of ſettling the latitudes of our ſtations is extremely accurate, will more fully appear from the following conſiderations. In the general computation of ſpherical triangles, a ſphere whoſe diameter is a mean between the longeſt and ſhorteſt of M. BOUGUER's ſpheroid has been adopted, becauſe it was obvious, that in our latitudes the degree of ſuch a ſphere could not differ ſenſibly from the mean degree of the ſpheroid. Thus the degree of the ſphere 60859.1 fathoms anſwers (as may be perceived by conſulting the table in the Paper of 1787) to the degree of the meridian on the ſpheroid in

the latitude of 51° 5′. Again, if the total length of one-fourth part of the ſpheroidical meridian of the earth, between the equator and the pole, 5478094.4 fathoms be divided by 90° (*Fig. de la Terre de* Bouguer, p. 310. and 311.), we ſhall have 60867.72 fathoms for the mean degree of the meridian, which in the ſame table will be ſeen not to differ ſenſibly from that anſwering to the latitude of Greenwich; in or near which parallel the curves of ſuch a ſphere and M. Bouguer's ſpheroid interſect each other, as will be readily conceived by referring to and conſidering the repreſentation of them, in Plate X. fig. 3.

Art. II. *Of the pole-ſtar obſervations in general.*

It became neceſſary, in the preceding article, to point out in what manner the latitudes of our ſtations have been deduced from their relative ſituation with regard to Greenwich; becauſe the method adhered to of ſettling the differences of longitude by the obſervations of the pole-ſtar, which could rarely be made except on one ſide, that is to ſay, at night, when the ſtar was eaſtward from the pole, implied as a matter of courſe, that the latitude of the ſtation ſhould be accurately known, for the computation of the ſtar's azimuth. With the declination of the ſtar, ſettled to ſo great a nicety as it has been by the Aſtronomer Royal, and the latitude of the place given, a ſingle azimuth was ſufficient for obtaining immediately the true direction of the meridian. Much time would have been uſeleſſly loſt in attempting to get obſervations of the ſtar in day-light when on the weſt ſide of the pole, whereby the double azimuth would have been obtained; and in that caſe the biſection of the angle would have given the true meridian of the place, without the knowledge of its latitude.

For the purpoſe of the pole-ſtar obſervations a ſmall table had been previouſly computed, of the exact times of the ſtar's being in the eaſt and weſt; whence the moments of its greateſt elongation were readily known. On theſe occaſions the Board of Longitude's *præmium watch*, by the late Mr. HARRISON, was made uſe of. Its rate of going all the time that it was in the field in 1787, was very uniformly 9½ ſeconds a day faſter than mean time. But in the winter months the watch gradually changed its rate from *plus* to *minus*, and when it was carried into the field in 1788, and, during the five weeks that it continued there, it regularly loſt on mean time from 3½ to 4 ſeconds each day; having in that ſhort interim been twice compared in Argyll-ſtreet, with an excellent clock made by CUMMING, with an improved ELLICOT's pendulum.

With regard to theſe pole-ſtar obſervations, whereby the differences of longitude, or the angles of convergence of the meridians towards each other, have been determined, it is neceſſary to remark, that although ſome few were made to the weſtward of Greenwich, yet theſe were not at ſufficient diſtance from it, and alſo taken of too ſhort ſides, to afford reſults that were perfectly ſatisfactory and concluſive. It is on the obſervations to the eaſtward only, and chiefly on thoſe made at Goudhurſt and Botley Hill, which are upwards of twenty-three miles from each other, and reciprocally viſible, that we have relied for the ſcale of degrees of a great circle perpendicular to the meridian in theſe latitudes; whence thoſe of longitude have been obtained. The obſervations made at Folkſtone Turnpike, which is upwards of fifty-eight miles in direct diſtance from Greenwich, and where, fortunately, the double azimuth of the pole-ſtar was obtained, are perfectly conſiſtent with thoſe taken at Goudhurſt and Botley Hill. But

when

when at Fairlight Down we had no obſervations of the ſtar, being at that time ſo much engaged with the other eſſential buſineſs of the triangles, and particularly with the interſection of the lights on the Coaſt of France, as to render it impoſſible to attend to any thing elſe, even if the weather had proved leſs unfavourable than it was at the period alluded to, for celeſtial obſervations.

ART. III. *Pole-ſtar obſervations at Goudhurſt and Botley Hill applied to computations on the mean ſphere.*

Let B (Plate X. fig. 5.) be Botley Hill; PBR its meridian; G Goudhurſt; W Wrotham Hill; T Tenterden; RG an arc of a great circle paſſing through G, and falling perpendicularly on the meridian BR; alſo let ∗∗ repreſent the circle of the pole-ſtar's apparent declination; and B∗, G∗, be two azimuth circles touching that circle.

Auguſt 14, 1788, at Goudhurſt, the angle ∗GT, or that between the pole-ſtar, when at its greateſt apparent diſtance from the pole on the eaſt ſide of the meridian, and the reverberatory lamp at Tenterden was obſerved †, . . 104 32 19½

The angle BGT, between the lamp at Botley Hill and Tenterden, was repeatedly obſerved, 167 43 56

Their difference = angle ∗GB is, . 63 11 36½

† The obſervations of the pole-ſtar at Goudhurſt and Botley Hill were repeated for ſeveral nights at each place; but theſe here given are the moſt exact. At Goudhurſt the angle which the ſtar made with the lamp being noted, the teleſcope removed, and the plane of the inſtrument being turned 180°, or half round, the teleſcope replaced and directed again to the ſtar, the difference on the circle was found to be only 1″½. The ſame method was univerſally adhered to in all places where obſervations of the ſtar were obtained. At Botley Hill, in particular, the difference between the readings was no more than 1″½.

Auguſt

	°	′	″
Auguſt 23, 1788, at Botley Hill, the angle ∗BW, or that between the pole-ſtar at its greateſt apparent elongation and the lamp at Wrotham Hill, was obſerved, . . .	76	21	37
The angle WBG, by repeated obſervations, was,	40	4	42
Their ſum = angle ∗BG is, . .	116	26	19

In order to obtain the ſtar's azimuth at each place, we may take, without producing any ſenſible error, the latitudes of G and B, as they would be found on M. Bouguer's figure, which we have already announced, and will hereafter prove to be conſiſtent with obſervation. Thus B, or Botley Hill, is ſouth from Greenwich 72882¾ feet, and nearly on the ſame meridian; wherefore its latitude will be 51° 16′ 41″.54, and its co-latitude BP of courſe is = 38° 43′ 18″.46. Now, P∗ the apparent diſtance of the ſtar from the pole at that time being = 1° 49′ 22″.84, in the right-angled ſpherical triangle P∗B, we have ſine BP : rad. :: P∗ : ſine 2° 54′ 54″.2 equal to the angle ∗BP, the ſtar's azimuth from the north. And this being added to the angle ∗BG obſerved 116° 26′ 19″, we have the angle ∗BP = 119° 21′ 13″.2 for that comprehended between the meridian and Goudhurſt.

The diſtance of Goudhurſt from the perpendicular to the meridian of Greenwich is 132592 feet, and its diſtance from the meridian of Botley Hill, on a perpendicular to that meridian, is 106171 feet nearly = GR. Hence the latitude of the point R is 51° 6′ 52″.89; therefore RP = 38° 53′ 7″.11, and RG = 106171 feet = 17′ 19″.7 nearly. Hence, as rad. : coſine RP :: coſine RG : ſine 51° 6′ 49″.7, the latitude of G nearly; therefore GP = 38° 53′ 10″.3; and P∗ the ſtar's apparent diſtance at the time being 1° 49′ 25″.34, we have the angle

PG

PG*, the ſtar's azimuth = 2° 54′ 20″.8, which being ſubtracted from the angle BG* obſerved at Goudhurſt between the lamp on Botley Hill and the ſtar, there remains the angle BGP = 60° 17′ 15″.7 comprehended at Goudhurſt, between Botley Hill and the meridian.

Now with theſe *data* let us ſuppoſe, in the firſt place, the earth to be a ſphere, whoſe diameter is a mean between the longeſt and ſhorteſt of M. BOUGUER's ſpheroids, the latitude of B, and of courſe its co-latitude BP, given; alſo the angles PBG and PGB reſpectively 119° 21′ 13″.2 and 60° 17′ 15″.7, we ſhall then have PG the co-latitude of G, and the angle BPG or difference of longitude of B and G. And becauſe the degree of ſuch a ſphere contains 60859.1 fathoms, the latitude of Botley Hill will then be 51° 16′ 41″.45, and BP its co-latitude = 38° 43′ 18″.55. This laſt ſide, with the former angles PBG and PGB reſpectively 119° 21′ 13″.2 and 60° 17′ 15″.7, give PG = 38° 53′ 6″.72 the co-latitude of G; and alſo the angle BPG, the difference of longitude of the points B and G equal to 27′ 36″.7. Again, in the right-angled ſpherical triangle PRG, rad. : tang. GP :: coſine of the angle RPG : tang. 38° 53′ 3″.47 = RP. But the point R is 22094 fathoms ſouth from Greenwich, and nearly on its meridian, therefore its latitude will be 51° 6′ 52″.8; and hence PR the co-latitude will be 38° 53′ 7″.2, which exceeds PR formerly found by ſpherical computation to be 38° 53′ 3″.47 by 3″.73, an arc equal to 63 fathoms. Alſo RG, the diſtance of Goudhurſt from the meridian of Botley Hill, on a perpendicular to that meridian, is equal nearly to 17695 fathoms, which, allowing 60859.1 fathoms for a degree, correſponds to an arc of 17′ 26″.7. But ſpherical computation formerly gave RG = 17′ 20″, the difference conſequently is 6″.7 = 1134 fathoms; therefore the earth cannot be this mean ſphere, which was aſſumed

aſſumed for the purpoſe of exemplification, becauſe its degrees, in the direction of the meridian, differ ſo little in theſe latitudes from thoſe of M. BOUGUER's ſpheroid.

ART. IV. *The ſame pole-ſtar obſervations applied to computations on a ſphere of greater dimenſions.*

Let us ſuppoſe, in the ſecond place, the earth to be a ſphere of ſuch magnitude as to have degrees of a great circle containing 61253½ or 61254 fathoms, we ſhall then get the latitude of B or Botley Hill = 51° 16′ 46″, the latitude of R = 51° 7′ 1″.2, and PR = 38° 52′ 58″.8; alſo RG = 17′ 19″.9. Now, BP = 38° 43′ 13″.9, and the obſerved angles will give the angle BPG, or the difference of longitude = 27′ 36″.7, the ſame as before, and the arc PG or co-latitude of G = 38° 53′ 2″.05 *. This laſt ſide, with the angle RPG = 27′ 36″.7 of the right-angled ſpherical triangle PRG, will give PR = 38° 52′ 58″.8, and RG = 17′ 19″.9; that is to ſay, the obſerved angles PBG and PGB, at Botley Hill and Goudhurſt reſpectively, are nearly the ſame as they would be found on a ſphere of ſuch magnitude as to have degrees containing 61253½ or 61254 fathoms. But ſince the value of RG as an arc of a great circle was before found by the triangles BPG and RPG to be 17′ 20″, when the latitude of B was taken as belonging to a ſphere whoſe degrees contained 60859.1 fathoms; and the ſame arc as now determined, *viz.* 17′ 19″.9, agrees very nearly

* It is evident, that as the latitude of B increaſes, the ſtar's azimuth, or the angle ✱BP, and conſequently the angle PBG, increaſe likewiſe. But at G the angle PGB is diminiſhed by the increaſe of the angle ✱GP, or the azimuth; and therefore if the difference of the latitudes of B and G remains the ſame, or nearly the ſame, the ſum of the angles PBG, PGB, will alſo be nearly the ſame; wherefore no ſenſible difference in the angle BPG, or difference of longitude, will be found on this account.

with the former, although the latitude of B be now taken on a ſphere whoſe degrees contain 61253¼ fathoms, it obviouſly follows, from theſe recent obſervations, *that whatever the preciſe figure of the earth may be, or the ratio between its diameters, the degree of a great circle upon it perpendicular to the meridian, cannot in theſe latitudes differ much in length from* 61253¼ *fathoms.*

ART. V. *The pole-ſtar obſervations at Folkſtone Turnpike applied to computations on the ſame greater ſphere.*

Let G (Plate X. fig. 6.) be Greenwich; PR its meridian; F, H, and T, the ſtations at Fairlight Down, High Nook, and Folkſtone Turnpike, reſpectively; alſo let PF and PT be meridians paſſing through F and T; and FR and Tr great circles cutting the meridian of Greenwich PR at right angles in R and r.

	°	′	″
At the ſtation T, on the 7th of September, 1788, at night, the angle between the pole-ſtar, when at its greateſt apparent elongation from the pole on the eaſt ſide of the meridian, and the reverberatory lamp at H, was obſerved, .	123	19	3¼
On the following morning, Sept. 8th, the angle between the ſtar, when at its greateſt diſtance on the weſt ſide, and the flag-ſtaff at H, was obſerved, . . .	117	30	52¼
The difference or double azimuth is, .	5	48	10¼
And the half ſum is, . .	120	24	57.87

This half ſum 120° 24′ 57″.87 * is the angle PTH, or that comprehended between the meridian PT and H. The angle HTF,

* By taking the latitude of T as determined on M. BOUGUER's ſpheroid, = 51° 5′ 45″.3 nearly, the co-latitude or TP is equal to 38° 54′ 14″.7, and the ſtar's apparent diſtance at the time being 1° 48′ 18″.03, we have, as ſine 38°

HTF, or that between the lamp at H and the white lights repeatedly fired at F, was twice obſerved 22′ 48″; therefore 120° 24′ 57″.87 + 22′ 48″ = 120° 47′ 45.87 is the angle PTF, that the ſtation on Fairlight Down makes with the meridian of Folkſtone Turnpike.

Now, *r*T being equal to 45827.88 fathoms, and RF = 23884.68 fathoms, if we take 61253¼ fathoms = 1°, we ſhall have G*r* = 22871 fathoms = 22′ 24″.18; GR = 36436.1 fathoms = 35′ 41″.42; *r*T = 44′ 53″.4; and RF = 23′ 23″.75; therefore PR, the co-latitude of R, will be 39° 7′ 1″.42; and that of *r* or P*r* will be 38° 53′ 44″.18. Hence, in the right-angled ſpherical triangle PRF, we ſhall have the angle RPF = 37′ 4″.901, and PF = 39° 7′ 7″.294. Further, the triangle P*r*T gives the angle *r*PT = 1° 11′ 29″.143, and PT = 38° 54′ 5″.98. Now, 1° 11′ 29″.143 − 37′ 4″.901 = 34′ 24″.242 = the angle FPT. This laſt angle, with the two containing ſides PT and PF, give the angle PTF = 120° 47′ 44″.75, the ſame as it was actually obſerved very nearly. And hence we have another ſtrong proof, *that on this part of our earth the degree of a great circle, perpendicular to the meridian, cannot differ much in length from* 61253¼ *fathoms, whatever may be its real figure, which cannot be determined until theſe obſervations ſhall have been compared with others that may hereafter be made in the ſame way, and with equal care, in latitudes remote from each other.*

38° 54′ 14″.7 : rad. :: ſine 1° 49′ 18″.03 : ſine 2° 54′ 5″.12 the ſtar's azimuth. Twice this angle, or 5° 48′ 10″.24, agrees very nearly with the double azimuth 5° 48′ 10″¾, found by the obſervations on the 7th and 8th of September. This near agreement, at the ſame time that it ſerves to ſhew the accuracy of theſe obſervations in particular, and the goodneſs of the mode that was adopted in general, ſerves alſo to prove, that Dr. MASKELYNE has ſettled the declination of the pole-ſtar to great preciſion.

Art. VI. *The latitude of the point* M *near Dunkirk, and conſequently the diſtance between the parallels of Greenwich and* M, *deduced from the ſame length of a degree perpendicular to the meridian. Alſo the compariſon of its length with that of the meridional degree.*

Again, let us ſuppoſe G (Plate X. fig. 7.) to be Greenwich; P*r* its meridian; M the point near Dunkirk, ſuppoſed to be in the meridian of Paris; M*r* a great circle paſſing through that point, and falling perpendicularly on P*r*. Then, if we take 61253½ fathoms = 1°, we ſhall have *r*M (= 89674·7 fathoms) = 1° 27′ 50″.37, and G*r* (= 25831.43 fathoms) = 25′ 18″.17. Hence P*r* will be 38° 56′ 38″.17; and therefore as rad. : coſine P*r* :: coſine *r*M : ſine 51° 1′ 58″.5 the latitude of M; and 51° 28′ 40″ − 51° 1′ 58″.5 = 26′ 41″.5 is the difference of latitude between Greenwich and M, or the diſtance of their parallels. Now, as 3600″ : 61253½ :: 26′ 41″.5 (= 1601″.5) : 27249·3 fathoms; and this being added to 133409.8 fathoms, the meaſured arc of the meridian between M and the Royal Obſervatory at Paris, we have 160659.1 fathoms for the length of the terreſtrial arc of the meridian comprehended between the parallels of the two Royal Obſervatories nearly. But the length of the celeſtial arc between them being 2° 38′ 26″ would, at the rate of 61253½ fathoms to a degree, give = 161743.3 fathoms, which exceeds the meaſured arc by 1084.2 fathoms. *Therefore it is ſufficiently obvious, that the earth cannot be a ſphere of theſe dimenſions; but it muſt be an oblate ſpheroid, on which a degree of a great circle, perpendicular to the meridian, in this way of conſidering it, exceeds in length the mean degree of the meridian between Greenwich and Paris in the proportion of* 61253½ *to* 60842, *or* 411½ *fathoms.*

Art.

ART. VII. *Application of the results of the pole-star observations to computations on M.* BOUGUER's *spheroid, for the distance of the parallels of Greenwich and* M.

Hitherto the results obtained by the geodetical measurement and pole-star observations have been applied to spherical computations on two spheres suited to the different lengths of degrees found in two opposite directions, at right angles to each other, the meridian and its perpendicular; and from these computations it has been clearly proved, that the earth cannot be either of the assumed spheres.

Let us therefore, in the next place, suppose the earth to have the figure and the dimensions of M. BOUGUER's spheroid, and by way of comparison apply the same results to computations on that figure. Thus the latitude of the point *r* will be found 51° 3′ 12″.09, and the arc *r*M = 1° 27′ 49″.03. Hence, as rad. : cosine *r*P :: cosine *r*M : sine 51° 1′ 48″.85 = the latitude of M nearly. Now, let the points *r* and M be represented by B and *v* (Plate X. fig. 2.) then will A represent the point whose latitude is 51° 1′ 48″.85; and by proceeding in the manner formerly directed for a spheroid, we get GW = 15.12 fathoms = to the distance in the axis between the points where the verticals from the latitudes 51° 3′ 12″.09 and 51° 1′ 48″.85 meet the said axis. Hence, as rad. : 15.12 (GW) :: cosine 51° 1′ 48″.85 (angle SWG) : 9.509 fathoms = GS, or the arc A*v* extremely near. Now the value of A*v*, as an arc of the meridian, is = 0″.56, which being added to 38° 58′ 11″.15 (AP), gives 38° 58′ 11″.71 = P*v*, the co-latitude of *v*; and hence the true latitude of *v*, or M (fig. 7.), is 51° 1′ 48″.29, which being subtracted from 51° 28′ 40″, the latitude of Greenwich, there remains 26′ 51″.71 for the arc between them, or distance of

their

their parallels, which on this ſpheroid correſponds to 27248.2 fathoms, leſs only by 1.1 fathom than the ſpace found, in the laſt article, to anſwer to an arc of 26′ 41″.5, being the diſtance of the ſame parallels on the greater ſphere.

Thus the meaſured length of the arc between Greenwich and M, 27248.2 fathoms, being added to the meaſured diſtance of M from the Royal Obſervatory at Paris, we have for the total length of the arc between Greenwich and Paris 160658 fathoms, which exceeds the computed length of the ſame arc on M. BOUGUER's hypotheſis by no more than 7½ fathoms.

But it hath been already ſhewn, that whatever the preciſe figure of the earth may be, a degree of a great circle upon it, perpendicular to the meridian, cannot in theſe latitudes differ much in length from 61253½ fathoms, being but 16½ fathoms leſs than 61270 fathoms, the length of the correſponding degree on M. BOUGUER's ſpheroid.

As far therefore as we are enabled to judge from the reſult of theſe obſervations, the earth differs but little either in its latitudinal or longitudinal dimenſions from what hath been aſſigned to it by M. BOUGUER.

ART. VIII. *Application of the pole-ſtar obſervations at Botley Hill and Goudhurſt, for determining the length of the degree of a great circle, perpendicular to the meridian.*

Since M. BOUGUER's ſcale for the degrees of the meridian hath been found to agree almoſt exactly with obſerved latitudes in this part of the earth, let us take the latitudes of B and R (fig. 5) as they would be found on his ſpheroid nearly, and apply the pole-ſtar obſervations at B and G, in order to find the length of the degree of a great circle, perpendicular to

the meridian of Botley Hill, paſſing through Goudhurſt. We ſhall then have PB, the co-latitude of Botley Hill, $= 38° 43' 18''.46$, and PR, the co-latitude of R, $= 38° 53' 7''.14$. Now, if the latitudes of B and R are nearly true, it follows, *that the point* G *muſt be ſomewhere in the great circle* RG, *whatever may be its longitude.* Therefore the angle BPG, or the difference of longitude between B and G, will be found in the following manner.

Augment the obſerved angle PBG $= 119° 21' 13''.2$, *and diminiſh the obſerved angle* PGB $= 60° 17' 15''.7$ *by the ſame quantity of a degree, until* PR *determined from the triangle* BPG *becomes* $= 38° 53' 7''.14$ *nearly; which will be when that quantity is* $9' 21''$. Thus the angles for computation will be $119° 21' 13''.2 + 9' 21'' = 119° 30' 34''.2$, and $60° 17' 15''.7 - 9' 21'' = 60° 7' 54''.7$; whence the angle RPG, or difference of longitude between B and G, will be found $= 27' 36''.75$, and the arc RG $= 17' 20''.06$ nearly $= 17695$ fathoms. *And hence the degree of a great circle, perpendicular to the meridian, of this new ſpheroid, will, in the latitude of* R, *contain* 61248 *fathoms nearly.*

This follows as a corollary from what hath been already ſaid concerning ſpheroidical triangles.

But ſince the difference of longitude between B and G was formerly determined to be nearly the ſame, *viz.* $27' 36''.7$, when the obſerved angles at theſe two ſtations, and alſo the latitude of B, were ſuppoſed to be on a figure different from this new ſpheroid; it therefore follows, *that the difference of longitude between any two ſtations* B *and* G, *diſtant in the preſent caſe from each other twenty-three miles* (and they ſhould never be much leſs remote) *may be found with ſufficient exactneſs, by having the horizontal*

horizontal angles at each ſtation obſerved very accurately, and the latitude of one of the ſtations given nearly.

The difference of longitude between Botley Hill and Goudhurſt, found as above, 27′ 36″.75, being augmented by the value of the ſmall arc comprehended between the meridians of Greenwich and Botley Hill = 2″.7, we have ultimately 27′ 39″.45 for the longitude of Goudhurſt, eaſtward from Greenwich.

ART. IX. *Difference between obſerved angles on the new ſpheroid and that of M.* BOUGUER.

Laſtly, on the ſubject of theſe compariſons, let us ſee what would be the difference between the obſerved angles at B and G, as determined on the new ſpheroid and on that of M. BOUGUER?

The latitudes of B and G on M. BOUGUER's ſpheroid would reſpectively be 51° 16′ 41″.54 and 51° 6′ 49″.66 nearly, and the angle BPG, or difference of longitude, would be = 27′ 36″.18. Now, this laſt angle, with the two co-latitudes PB and PG, as containing ſides, and ſuppoſed to form a ſpherical triangle, will give the angles at B and G reſpectively 119° 31′ 26″.47 and 60° 7′ 3″.18. But the obſerved angles at theſe ſtations would be 119° 21′ 32″.97 and 60° 16′ 56″.68, the common difference between them being 9′ 53″.5, which is 32″.5 greater than 9′ 21″, as was before determined. Hence we may conclude, that in this new ſpheroid, founded immediately on the recent geodetical meaſurements and obſervations of the pole-ſtar made at Botley Hill and Goudhurſt, the verticals from B and G meet the earth's axis at a leſs diſtance from each other than they would in M. BOUGUER's ſpheroid. The length of the vertical is ſhorter as well as the radius of the parallel, whereby Goudhurſt,

hurſt, or the point R, is leſs removed from the earth's axis than it would be on the former figure; and conſequently it is probable, that the ſpheroid is leſs oblate.

From the preceding determinations it is further evident, that ſuppoſing the latitudes of B and G, with the horizontal angles PBG and PGB to be given by obſervation, not only the difference of longitude, or the angle BPG, will be obtained, but alſo the arc BR of the meridian, the arc RG of a great circle perpendicular to it, and the oblique arc BG, all conſidered as arcs of great circles of the ſpheroid.

ART. X. *Further illuſtration of the manner of ſettling the latitudes and longitudes of the ſtations comprehended in the general table of reſults.*

Having ſhewn, in the preceding part of this ſection, how the length of the degree of a great circle, perpendicular to the meridian, and alſo the differences of latitude and longitude, have been obtained by very accurate obſervations of the pole-ſtar made at certain ſtations to the eaſtward of Greenwich, whereby we have been furniſhed with a ſcale for ſettling the longitudes of all the other ſtations where no obſervations of the pole-ſtar could be had, or only ſuch as were not to be depended upon; we ſhall, by way of further illuſtration of this matter, give another example of the calculations for the point M near Dunkirk, which will ſuffice for all the other ſtations comprehended in the general table of reſults placed at the end of this ſection, where the reſpective columns have been filled up by the ſame or a ſimilar mode of computation.

Let G (Plate X. fig. 8.) be Greenwich; GR*r* its meridian; G*g* the perpendicular to that meridian, produced eaſtward; MR a parallel to that perpendicular drawn through the point

M; and let Mg be a portion of a ſmall circle of the ſpheroid, or parallel to the meridian of Greenwich, produced from M northward, until it interſects the perpendicular in the point *g*. Alſo, let MP repreſent the meridian of the Royal Obſervatory at Paris, paſſing through the point M, and interſecting the parallel of Greenwich in P. Further, let C repreſent the church of *Notre Dame* at Calais, and making, as appears by the triangles, an angle RMC of 14° 51′ 3″.9 with the parallel to the perpendicular of the meridian of Greenwich drawn through the point M.

From the annexed general table of the reſults of the triangles, it appears, that MR=*g*G contains 538048 feet= 89674.7 fathoms; and that GR=*g*M contains 154938 feet= 25823 fathoms. Now, ſince great circles, perpendicular to any meridian of the ſpheroid, converge towards each other, as they depart from that meridian, in the ſame manner as the meridians themſelves do in departing from the equator, but by a ſlower rate, it is obvious, that the perpendicular to the meridian of Greenwich, paſſing through the point M, muſt fall below or to the ſouthward of R on that meridian, ſo as that G*r* : GR :: rad. : coſine MR = 1° 27′ 51″, conſidered as a portion of a great circle of the ſpheroid, perpendicular to the meridian of Greenwich. Hence, G*r* will contain 25831.43 fathoms = 25′ 27″.9 of latitude, and therefore the latitude of *r* will be 51° 3′ 12″.1, and its co-latitude 38° 56′ 47″.9. Alſo, R*r* meaſures 8.43 fathoms, and ſubtends an angle RM*r*= 19″.42.

In the right-angled ſpherical triangle, pole *r*M, right-angled at *r*, making uſe of the half ſum and half difference of the containing ſides, *r* pole and *r*M, with the co-tangent

gent of 45° we have the angle of longitude r pole M, ... = 2 19 42.5

The angle, pole Mr, . . . = 88 11 21.4

And the complement of this laſt, or the convergence of the meridian of M (ſuppoſed to coincide with that of Paris) to the meridian of Greenwich, ... = 1 48 38.6

Alſo, as ſine 88° 11′ 21″.4 : ſine 38° 56′ 47″.9 :: rad. : ſine 38° 58′ 11″.2 the co-latitude of M; whence its latitude becomes 51° 1′ 48″.8, from which, deducting the ſpheroidical correction 0″.5, we have the true latitude of M = 51° 1′ 48″.3. The difference between this and the latitude of r = 51° 3′ 12″.1 will be 1′ 23″.8, anſwering on the ſpheroid to 1416.77 fathoms; and this laſt number being added to the value of the arc Gr = 25831.43 fathoms, we have 27248.2 fathoms for MP, or the diſtance of the parallels of Greenwich and M on this new ſpheroid.

Laſtly, if to this mean diſtance of the parallel of M from Greenwich, we add the mean diſtance of the parallel of M from Paris = 133398.8 fathoms, we ſhall then have 160647 fathoms for the mean diſtance of the parallels of Greenwich and Paris, anſwering to the celeſtial arc of 2° 38′ 26″. Hence the mean degree of the meridian between Greenwich and Paris correſponding to the latitude of 50° 9′½, contains 60838.3 fathoms, or about 1¼ fathom leſs than M. Bouguer's degree for the ſame latitude.

ART. XI. *Compariſon of the angle between the meridian of the point* M *and a line drawn from thence to Calais, as approximately deduced from the Britiſh and French obſervations.*

In the ſpheroidical quadrilateral G*g*r**M** (fig. 8.), formed by three arcs of three great circles, and one of a ſmall circle of the ſpheroid, we have two right angles at G and *r*, and two others at *g* and M, each greater than a right angle by 9″.7; therefore the angle RMC, reſulting from the triangles = 14° 51′ 3″.9 − RM*r* (19″.42) = 14° 50′ 44″.5 + 90° 0′ 9″.7 (CM*g*) = 104° 50′ 54″.2, is the angle *g*MC, or that which Calais makes with a parallel to the meridian of Greenwich drawn through the point M. From this laſt angle ſubtracting the angle PM*g* = 1° 48′ 38″.6, or the quantity by which the meridian of M (ſuppoſed to co-incide with that of Paris) converges towards that of Greenwich, there remains the angle PMC = 103° 2′ 15″.6 for the angle that the meridian of M ſhould make with a line drawn from D, or Dunkirk, through that point to Calais, according to the Britiſh obſervations.

By the late French operations, the meridian of Dunkirk makes, with a line drawn through M to Calais, an angle of 102° 59′ 51″.5. The convergence of the meridian of M to that of Dunkirk, on a difference of longitude of 2′ 21″.54, is 1′ 49″.94, which being added to 102° 59′ 51″.5, we have 103° 1′ 41″.44 for the angle that the meridian of M, or of Paris, makes with a line drawn from Dunkirk through that point to Calais. The difference between the two reſults 34″.16 is nearly equal to the mean of two extremes $\left(\frac{1' 15''}{2}\right) = 37''\frac{1}{2}$, the apparent uncertainty, in the determination of that angle

by two ſets of angles given in the *Méridienne vérifiée*, as adverted to in the Paper of 1787, Phil. Tranſ. Vol. LXXVII. p. 195, 196.

ART. XII. *The longitudes of Dunkirk and Paris, eaſtward from Greenwich, determined by the ſum of four differences of meridians.*

In fig. 9. let PA be the meridian of Greenwich; G Goudhurſt, PR its meridian; T the ſtation at Folkſtone Turnpike, PS its meridian; C Calais, PC its meridian; D Dunkirk, and PB its meridian. Alſo, let AG, RT, SC, and BC, be arcs of great circles, making the angles PAG, PRT, PSC, and PBC, right ones.

The angle at Goudhurſt, between its meridian and Tenterden, is 107° 26′ 40″.3; hence, by drawing parallels to this meridian through Tenterden and the ſtation at Allington Knoll (ſee the plan of the triangles) we ſhall get 946.6 fathoms for what the ſtation at the Turnpike is ſouthward, and 28098.8 fathoms for what it is eaſtward from the meridian of Goudhurſt. Now, 60859.4 fathoms being nearly = 1° of the meridian in the latitude of Goudhurſt, we have 946.6 fathoms = 56″ nearly = the arc GR; and the latitude of Goudhurſt being 51° 6′ 49″.6, that of the point R is 51° 5′ 53″.6; hence the co-latitude RP = 38° 54′ 6″.4; and ſince the degree of a great circle, perpendicular to the meridian, in this latitude has been ſhewn to contain 61248 fathoms nearly; therefore RT = 28098.8 fathoms will be 27′ 31″.6. This arc and RP give the angle RPS = 0° 43′ 49″.86 for the difference between the meridians of Goudhurſt and Folkſtone Turnpike.

The angle at Folkſtone Turnpike between its meridian and Dover was obſerved 66° 48′ 35″, and if we draw a parallel to this meridian through Dover, we ſhall find, that Calais is

25284.2

25284.2 fathoms eaſtward from the meridian of the Turnpike. Now, the latitude of Calais being 50° 57′ 30″ nearly (which is accurate enough for computation) the length of the degree of a great circle, perpendicular to the meridian in that latitude, will be 61246 fathoms nearly. Hence, 25284.2 fathoms = 24′ 46″.8 = the arc CS; this, with the co-latitude CP (39° 2′ 30″), give the angle CPS = 39′ 19″.48, for the difference of longitude between the Turnpike and Calais.

By Comte de Cassini's Paper, communicated in January 1789, it appears, that the angle at Dunkirk, between its meridian and Broulezele, is 10° 18′ 25″; and that between Broulezele and Calais 66° 41′ 46″½, the ſum is 77° 0′ 11″½ for the angle at Dunkirk, between its meridian and Calais. In the ſame Paper we have 19349 34 toiſes for the diſtance of Dunkirk from Calais; this, with the angle 77° 0′ 11″½, give 18853.7 toiſes or 20093.3 fathoms for the diſtance of Calais weſtward from the meridian of Dunkirk, which, by taking 61246 fathoms = 1° (that of a great circle perpendicular to the meridian in the latitude of Calais), is equal to 19′ 41″.1 = the arc BC; and this arc, with CP the co-latitude of Calais, give the angle CPB = 31′ 15″.11 for the difference between the meridians of Calais and Dunkirk.

The angle APR, or the difference of the meridians of Greenwich and Goudhurſt, has already been found (ſee the end of the 8th article, and alſo the table of general reſults), . . = 0 27 39.45

The angles	RPS	= 0	43 49.86
	SPC	= 0	39 19.48
	CPB	= 0	31 15.11

Hence the total angle APB, or long. of Dunkirk, is = 2 22 3.9

It

It hath been already remarked, that, from p. 276. *Méridienne vérifiée*, Dunkirk is 1430 toiſes eaſtward from the meridian of Paris; and that in p. 36. of the *Déſcription Géometrique de la France*, we find it only 1416 toiſes. Now, theſe will give 2′ 22″.6 and 2′ 21″.2 reſpectively for the difference of meridians of Dunkirk and Paris; the mean is 2′ 21″.9 for the longitude of Dunkirk eaſt from Paris; therefore 2° 22′ 3″.9 − 2′ 21″.9 = 2° 19′ 42″, or 9′ 18″.8 in time, will be the longitude of Paris eaſt from Greenwich nearly.

Again, in fig. 10. let P be the pole; G Greenwich, PW its meridian; RD an arc of a great circle making the angle at R a right one, and paſſing through D; and DW an arc of the parallel of latitude of Dunkirk.

By p. 240. of the *Mem. de l'Acad.* 1758, the celeſtial arc between Paris and the ſtation of the ſector near Dunkirk is 2° 11′ 50″; to which adding 5″.3 (= 84¾ toiſes) for what the tower is north from the ſtation, we have 2° 11′ 55″.3 for the arc between Paris and Dunkirk; therefore, if the latitude of Paris is 48° 50′ 14″, that of Dunkirk will be 51° 2′ 9″.3, whence its co-latitude becomes 38° 57′ 50″.7 = DP.

From what has been ſaid concerning ſpheroidical triangles, it follows, by way of corollary, that to find RD by ſpherical computation, when DP and the angle at P are given, it is neceſſary to diminiſh DP by a certain quantity determinable from the nature of the ſpheroid; this quantity is about 0″.5 when the ſpheroid is M. Bouguer's; therefore DP may be taken = 38° 57′ 50″.2, which is ſufficiently accurate for computation.

Hence, as rad. : ſine DP :: ſine 2° 22′ 3″.9 = WPD : ſine 1° 29′ 19″.17 = the arc DR. Now, 61247 fathoms being equal to 1° of a great circle perpendicular to the meridian in the

the latitude of Dunkirk nearly, we have, as 1″ : 61247 :: 1° 29′ 19″.17 : 91175.8 fathoms = the arc DR.

But the length of this arc DR has been found nearly the ſame, that is, 91176.3 fathoms (ſee the table of general reſults) by continually drawing parallels to the meridian of Greenwich through the different ſtations between it and Dunkirk; therefore, although that method in general is not ſtrictly accurate, having a tendency to give the reſults in exceſs; yet it is evident, that the length of the arc of a great circle ſo determined will differ very little from the truth, when the ſeries of triangles employed for that purpoſe are contiguous to it, and follow its direction nearly.

ART. XIII. *For the diſtance between the parallels of latitude of Greenwich and Paris.*

The diſtance between the parallels of Greenwich and Paris has already been determined in Art. X. of this ſection, by taking M (1420.41 toiſes weſtward from Dunkirk) as the intermediate point. Let us next ſee what will be the reſult when Dunkirk is made uſe of inſtead of M?

In fig. 10. as cof. RD : rad. :: cof. DP : cof. PR = 38° 56′ 24″.07; but DP by obſervation is = 38° 57′ 50″.7 = PW; hence PW − PR = 38° 57′ 50″.7 − 38° 56′ 24″.07 = 1′ 26″.63 = RW = 1464.5 fathoms nearly, by taking 60858 fathoms for a degree of the meridian, that being nearly its value in the latitude of Dunkirk.

By our operation Dunkirk is 25425 fathoms ſouthward from Greenwich; but the great circle DR meets the meridian of Greenwich about 8½ fathoms further ſouth, that is to ſay, GR is 25425 + 8.5 = 25433.5 fathoms, which being added to 1464.5 gives

gives 26898 for the diſtance between the parallels of latitude of Greenwich and Dunkirk.

Becauſe Dunkirk is ſituated near the meridian of Paris, the diſtance between the parallels of latitude of theſe places will be nearly equal to what Dunkirk is north from Paris, namely, 125517¼, or 125495 toiſes (ſee the pages formerly quoted). Theſe numbers give reſpectively 133770 + 26898 = 160668, and 133746.3 + 26898 = 160644.3 fathoms for the diſtances between the parallels of Greenwich and Paris, a mean between which will nearly be 160656 fathoms.

If therefore the celeſtial arc of the meridian between Greenwich and Paris is 2° 38′ 26″, we get 60846¼ or 60837¼ fathoms for a degree of the meridian in latitude 50° 9′¼, the middle point between Greenwich and Paris; and a mean of theſe two reſults 60841¼ only exceeds M. Bouguer's degree for the ſame latitude about 1¼ fathom, a quantity not differing ſenſibly from the defect that was brought out by the computation in Art. X. Finally, therefore, by taking a mean between this and the former length 60838¼, we ſhall have 60840 fathoms for the degree of the meridian in latitude 50° 9′ 27″, agreeing almoſt exactly with that of M. Bouguer.

Art. XIV. *Compariſon of the length of a degree of a great circle, perpendicular to the meridian in Kent, with that in the South of France.*

M. Caſſini de Thury, in his Book *La Méridienne vérifiée*, has given us the detail of an operation carried on in the South of France in latitude 43° 32′, for the determination of the length of a degree of longitude, by marking, at the extremities of a long and well aſcertained diſtance, the inſtantaneous exploſion of gunpowder in the open air. For this purpoſe a

ſeries of triangles was extended along the ſhore of the Mediterranean Sea, between Cette and St. Victoire, the extreme ſtations from whence the light was repeatedly obſerved, as fired at the church of St. Maries, nearly in a central ſituation, at the mouth of the leſſer branch of the river Rhone.

From the reſult of this operation, the beſt of the kind that has ever been executed in any country, it appears, that a degree of longitude in that latitude meaſures 44355.7 fathoms; whence it follows, that the degree of a great circle, perpendicular to the meridian there, muſt contain 61182¼ fathoms, being 65¼ fathoms leſs than the degree in the middle of Kent, latitude 51° 6′ 50″.

Now, if we compare this difference with that found between the correſponding degrees of great circles for the ſame latitudes on M. BOUGUER's hypotheſis, we ſhall find them perfectly conſiſtent with each other in their rate of diminution: for, by conſulting the table, it will be ſeen, that this degree in latitude 51° 6′ 50″ exceeds that in latitude 43° 32′ by 64.7 fathoms, agreeing within leſs than a fathom with the former difference.

On due conſideration of ſo many corroborating circumſtances as have been adduced in the courſe of this ſection, there ſeems, therefore, to be ſufficient room to conclude, that the earth differs but little either in its figure or dimenſions from what hath been aſſigned to it by M. BOUGUER. It is true, indeed, that a new ſpheroid has been here preſented, ſomewhat leſs flat than the former, founded immediately on the Britiſh obſervations; and theſe being again compared with the reſult of the above-mentioned operation, whereby the degree of longitude in the South of France was determined, it is from the combination of both reſults that the annexed table of the lengths

lengths of degrees of great circles and of longitude has been constructed for middle latitudes only, extending from 42° to 52°. Without the help of such a table, the new longitudes of some intermediate places, which we shall have occasion hereafter to compare with the old, could not have been so accurately computed as was wished. Now, although it is believed, that this table will be found to answer nearly in that zone of the earth for which it is intended; yet it is only offered for temporary use, until future observations of the pole-star in the same parallel, but on longer distances than our recent series of triangles afforded, or the extension of operations of the same nature with ours into remoter latitudes, shall have furnished *data* for one more correct.

Table of the degrees of great circles and of longitude for middle latitudes.

Places.	Latitudes.			Deg. of great circles, perp. to the meridian in fath.	Diff.	Degrees of longitude in fathoms.	Diff.
	°	′	″				
	42	0	0	61170.5	7.5	45458.5	715.7
	43	0	0	61178.		44742.8	
South of France	43	32	0	61182.5	4.5	44355.7	387.1
	44	0	0	61186.5	4.	44013.9	341.8
	45	0	0	61195.	8.5	43271.4	742.5
	46	0	0	61203.	8.	42515.2	756.2
	47	0	0	61211.5	8.5	41746.1	769.1
	48	0	0	61220.	8.5	40964.2	781.9
Paris Royal Observatory	48	50	14	61227.5	7.5	40303.2	662.0
	49	0	0	61229.	1.5	40169.8	132.4
	50	0	0	61237.5	8.5	39362.8	807.0
	51	0	0	61246.5	9.	38542.7	820.1
M near Dunkirk	51	1	48.3	61246.7	0.2	38518.8	23.9
Dunkirk	51	2	9.3	61246.75	0.05	38514.1	4.7
Middle of Kent	51	6	49.6	61248.	1.25	38450.0	64.1
Greenwich Royal Observ.	51	28	40	61251.1	3.1	38148.7	301.3
	52	0	0	61255.	3.9	37712.3	436.4

51.28.40
43.30.14
Diff. 2.58.26

ART. XV. *Compariſon of the old longitudes of ſome places on the ſkirts of the kingdom of France with what they will be when computed by the new* data.

If the preceding determinations of the longitudes of the ſeveral ſtations between Greenwich and Dunkirk are accurate, or nearly ſo, as founded immediately on the Britiſh obſervations, and ultimately combined with the reſult of the operation in latitude 43° 32′, it follows, that all the longitudes of the great map of France, the labour of more than half a century, will be conſiderably affected thereby, in proportion to the diſtances of the places, eaſtward or weſtward, from the meridian of the Royal Obſervatory at Paris reſpectively.

To ſhew the effect produced by the new *data*, we ſhall collect, in the following table, the latitudes and old longitudes of a few noted places on the ſkirts of that great kingdom, and annex to them the new longitudes reſulting from computations made with new lengths of degrees of great circles, perpendicular to the meridian, correſponding to their latitudes reſpectively. It will readily be conceived, that the object here in view is ſolely this; namely, that aſtronomers who live near thoſe places, and who have their time, that is to ſay, the directions of their meridians very accurately aſcertained, may, by their future correſponding obſervations (which ſhould only be occultations of the fixed ſtars behind the moon's dark limb) compare the old with the new longitude, and thus be enabled to ſatisfy the curious world, which of the two comes neareſt to the truth.

Com-

Comparative table of the old and new longitudes of ſome noted places on the ſkirts of the kingdom of France.

	Places.	Latitudes.	Longitudes, Old.	Longitudes, New.	Diff. of old and new long. in deg^s &c.	Diff. of old and new long. in time.
		° ′ ″	° ′ ″	° ′ ″	′ ″	Sec. Thirds.
East from Paris.	Pilier de Cette	43 24 6	1 21 7	1 20 3	0 23	1 32
	Tour de Planier, near Marſeilles	43 11 58	2 54 8	2 53 6	1 2	4 8
	Signal of St. Victoire	43 31 52	3 15 8	3 13 58	1 10	4 40
	Straſbourg (Conn. de T. 1788)	48 34 35	5 26 18	5 24 6	2 12	8 48
	Ditto (Déſcrip. Géomét. 1783, p. 171.	48 34 50	5 25 0	5 23 33	1 27	5 48
West from Paris.	Tour de Cordouan at the mouth of the Garonne, Conn. de T. 1788	45 35 15	3 30 38	3 29 18	1 20	5 20
	St. Malo	43 39 0	4 22 22	4 20 37	1 45	7 0
	Fort du Pilier, at the mouth of the Loire	47 2 29	4 42 20	4 40 30	1 50	7 20
	Uſhant Light-houſe	48 28 30	7 24 33	7 21 41	2 52	11 28
	The greateſt difference between Straſbourg and Uſhant				5 4	20 16
	The leaſt difference				4 19	17 36
	The mean difference				4 41½	18 46

With regard to the longitudes in the preceding table, it is only neceſſary to obſerve, that the two books of 1744 and 1783, ſo often quoted, are not always conſiſtent with each other, and both do ſometimes diſagree with what has been placed on the margin of the map of France. It would ſeem, that in the *Déſcription Géométrique* a ſcale for degrees of longitude has been uſed conſiderably greater than that correſponding with the ſpherical hypotheſis adhered to in the conſtruction of the map, yet ſtill too ſmall for what we have found to be their meaſure in Kent, or that reſulting from their own operations in the South of France. But if a ſimilar mode to that which they practiſed with ſo much ſucceſs in the South had

had been employed in the North of France, the ſame ſort of reſult as we have obtained in Kent would probably have been the conſequence; in which caſe it cannot be doubted, that the ſpherical hypotheſis would have been entirely rejected, and their lengths of degrees of longitude would have been ſuited to an oblate ſpheroid, whoſe degrees of the meridian and of great circles perpendicular to it had nearly the proportion to each other of 60840 to 61239 for the middle latitude between Greenwich and Paris, being an exceſs of 399 fathoms on each degree in the longitudinal direction.

On the whole, therefore, as matters ſtand at preſent, it is ſufficiently obvious, that, in the total extent of the kingdom of France from Straſbourg on the eaſt to Uſhant on the weſt, the difference between the old and new longitude amounts to between 17 and 20 ſeconds of time; that is to ſay, the real difference between the meridians of thoſe places, it is preſumed, will not be found by future obſervations made on the occultations of the fixed ſtars, to be ſo great as it was formerly ſuppoſed to be by that quantity, or ſomething approaching it very nearly.

ART. XVI. *The obſervations of eclipſes cannot be depended upon for determining with ſufficient accuracy the difference of longitude in vicinal ſituations.*

Finally, with regard to differences of longitude, it may not be improper in this place to remark, that, in vicinal ſituations, ſuch as Greenwich and Paris, the eclipſes of the ſun and moon and Jupiter's ſatellites do not, in general*, give reſults

* The reſult deduced by the Profeſſor PIAZZI, of the Univerſity of Palermo, from the obſervations of the eclipſe of the ſun on the 3d of June, 1788, made at Greenwich, in company with Dr. MASKELYNE and M. D'ARQUIER, as given in the Phil. Tranſ. for 1789, p. 58. is an exception well worthy of notice.

ſufficiently near the truth to deſerve even the name of an approximation. This will inconteſtably appear by comparing the aſtronomical reſult produced in that way with ours obtained by actual meaſurement on the ſurface of the earth, and angular obſervations of the pole-ſtar. Thus, by taking a mean of a multitude of the beſt of theſe obſervations of eclipſes, &c. collected and corrected with great care for the purpoſe, the difference in time between Greenwich and Paris amounts to 9′ 30″½ *, inſtead of being only 9′ 19″ nearly, which our operation makes it. Now, if the difference in time between theſe two Royal Obſervatories was really ſo much, the degree of a great circle perpendicular to the meridian in theſe latitudes (51° 6′ 50″ that of Goudhurſt, or 51° 1′ 48″ that of the point M, it matters little which of the two is taken) would be between 1200 and 1300 fathoms ſhorter than the degree of the meridian in the ſame latitudes. Hence the earth, inſtead of being an oblate ſpheroid conſiderably flattened at the poles, would be one extremely prolate, in proportion with regard to the former figure of more than three to one, or between 1200 and 1300 — to about 400 +.

In remote ſituations, ſuch as Europe and America, Europe and the eaſtern parts of Aſia, ſeparated from each other by wide oceans, the differences of longitude can only be obtained by means of aſtronomical obſervations. And as theſe will always be liable to ſome error, which may be as great on a difference of one or two, or a few degrees, as on the whole 180°, it is ſufficiently obvious that, to render the effect of ſuch

* From Dr. MASKELYNE's Paper of 1787, Phil. Tranſ. p. 183. it appears, that the eclipſes of the 1ſt ſatellite of Jupiter give immediately for the difference of meridians of the two Obſervatories 9′ 30″½, without being combined with obſervations made in other parts of Paris.

error

error as ſmall as poſſible, occultations of fixed ſtars ſhould only be made uſe of, for obtaining concluſive determinations.

In vicinal ſituations, the next beſt mode to angular meaſurement is no doubt that of marking, by means of well-regulated clocks, as was done in the South of France, the repeated inſtantaneous exploſion of light, obſerved at ſtations as far diſtant to the eaſtward and weſtward of the place of exploſion as the circumſtances will permit in practice, theſe diſtances having been for the purpoſe accurately ſettled by trigonometrical operation. The preferable ſtations for experiments of this ſort will be pointed out in the concluſion to the preſent Memoir.

TABLE

TABLE containing the GENERAL RESULTS

	Stations.	By Plane Trigono		
		Distances in Feet.		Bearin
		from the Merid. of Greenwich.	from the Perp. to the Merid	Angle w Meri
		. . . .	. . . .	. . .
	Greenwich R. Ob. Tranſit Room			o /
West from Greenwich	Norwood	19306.54	24603.86	38. 7.1
	Hundred Acres	43333.9	50937.9	40.23.1
	Hanger Hill Tower	67740.69	16729.21	76. 7.4
	Hampton Poor-houſe	83086.16	18537.98	77.25.2
	King's Arbour	102264.55	1037.69	89.25.
	St. Ann's Hill	119404.04	28852.77	76.24.5
	Wardrobe Tower of Windſor Caſtle	137050.54	2562.72	88.55.
East from Greenwich	Botley Hill	171.6	72882.5	0. 8.
	Severndroog Caſtle on Shooter's Hill	14032.3	4069.8	73.49.5
	Frant	62342.5	138460.4	24.14.2
	Wrotham Hill	71850.3	59305.8	50.27.4
	Goudhurſt	106342.5	132592.	38 43.5
	Fairlight Down	143308.1	218611.6	33.14.4
	Hollingborn Hill	151078.1	77077.6	62.58.1
	Tenterden	158317.6	148567.	46.49.1
	Ruckinge	204801.6	149410.3	53.53.1
	Lydd	209339.3	190695.6	47.40.6
	Allington Knoll	219926.3	144032.3	56.46.4
	High Nook near Dymchurch	228246.4	165672.9	54. 1.3
	Padleſworth	261707.6	130836.4	63.26.
	Swingfield	273722.7	118730.9	66.33.2
	Folkſtone Turnpike	274967.3	137214.2	63.28.4
	Dover Caſtle, north Turret of the Keep	303766.8	124319.1	67.44.
	Montlambert near Boulogne	382889.6	273450.3	54.27.
	Blancnez	394891.7	197153.5	63.28.3
	N. D. at Calais	427456.6	184263.3	66.40
	Point M near Dunkirk, Merid. R. Ob. Paris	538048.2	154938.2	73.56.
	Dunkirk	547058.1	152549.1	74.25.

[To face page 232.]

LTS of the TRIGONOMETRICAL OPERATION.

igonometry.

Bearing or ngle with the Meridian.		Direct Dist. from Greenwich.	Latitude.	Longitude.		Vertical Heights.		
				in Degrees, &c.	in Time.	Ground above the Sea	Telescope above Ground.	Total.
° ′ ″		Feet.	° ′ ″	° ′ ″	m. s. th.	Feet.	Feet.	Feet.
. . .	. .	. . .	51.28.40			170.5.	43.5	214
3. 7.16	SW	31274-48	51.24.37.34	0. 5. 2.4	0.20. 9.6	380.3	9.2	389.5
0.23.18.5	SW	66876.73	51.20.17.3	0.11.19.5	0.45.18.	433.8	9.2	443
6. 7.40.2	NW	69775.8	51.31.24.16	0.17.46.5	1.11. 6.	213.	38.	251
7.25.20.3	SW	85129.1	51.25.35.2	0.21.45.3	1.27. 1.2	63.5	37.5	101
9.25. 7.7	NW	102269.8	51.28.47.16	0.26.48.5	1.47.14.	94.8	37.5	132.3
6.24.55.7	SW	122840.6	51.23.51.4	0.31.14.7	2. 4.58.8	321.3	20.7	342
8.55.43.5	NW	137074.5	51.28.59.7	0.35.55.8	2.23.43.2			
0. 8. 5.6	SE	72882.7	51.16.41.54	0. 0. 2.7	0. 0.10.8	859.3	20.7	880
3.49.34	SE	140.10.58	51.27 59.8	0. 3.40.65	0.14.42.6	417.8	64.2	482
4.14.23.6	SE	151848.2	51. 5.53.9	0.16.12.5	1. 4.50.	604.1.	54.9	659
0.27.48.5	SE	93164.6	51.18.53.86	0.19.12.8	1.16.49.2	761.8	9.2	771
8 43.50.1	SE	169968.8	51. 6.49.62	0.27.39.45	1.50.37.8	437.9	59.1	497
3.14.46.8	SE	261396.7	50.52.38.84	0.37. 5.	2.28.20.	539.5	5.5	599
2.58.12.2	SE	169604.1	51.15.53.5	0.39.25.2	2.37.40.8	611	5.5	616.5
6.49.11.4	SE	217109.7	51. 4. 8.15	0.41. 8.15	2.44.32.6	220.6	101.7	322.3
3.53.16.3	SE	253509.6	51. 3.54.9	0.53.12.6	3.32.50.4	31.9	5.5	37.4
7.40.6	SE	283174.5	50.57. 7.4	0.54 15.4	3.37. 1.6	31.7	98.7	130.4
6.46.43.5	SE	26289 .3	51. 4.46.1	0.57. 9.4	3.48.37.6	323.5	5.5	329
4. 1.33.4	SE	282035.3	51. 1.11.7	0.59.14.6	3.56.58.4	22.1	5.5	27.6
3.26.16.8	SE	292590.2	51. 6.50.36	1. 8. 9.	4.32.36.	621.3	20.7	642
6.33.2	SE	298364.1	51. 8.47.8	1.11.14.6	4.44.58.4	473.1	56.9	530
3.28.47.6	SE	307302.3	51. 5.45.3	1.11.29.3	4.45.57.2	569.8	5.5	575.3
7.44 34	SE	328222	51. 7.47.7	1.19. 2.1	5.16. 8.4	373.9	95.1	469
4 27.59.4	SE	470509.8	50.43. 2 3	1.38.45.	6.35. 0.	639.5		
3.28.8	SE	441371.7	50.55.31 3	1.42 17.9	6.49.11.6	444.5		
6.40 50.2	E	465480.5	50.57.30.67	1.50.48.8	7.23.15.8	140.5		
3.56. 7.2	SE	559912.8	51. 1.48.3	2.19.42.5	9 18.50.			
4.25. 7.2	SE	567929.4	51. 2. 9.3	2.22. 4.	9.28.16.			

SECTION.

For correcting the TABLE in the

		°	′	″	‴	°	′	″	‴	°	Pts.										
1	Greenwich Rl. Ob.	51	28	40	00	}	8	46	48	00	8.	78		534343.			535848.		535758.		535024.
	Perpignan - -	42	41	52	00																
	Greenwich and M	MP				diff.	0	26	50	52	0.	4475		27235.			27342.		27337.		27294.
2	Point M near Dunkirk	51	1	49	8	}	8	19	57	8	8.	3325	506687	507108.	+521	Mean error on each deg. + 62.53	508506.	+1819	508421.	+1734	507730.
	Perpignan - -	42	41	52	00																
	Point M and Paris -					diff.	2	11	35	8	2.	1931	133409	133467.	+ 58		133957.	+ 584	133932.	+ 523	133732.
3	Paris Rl. Ob. -	48	50	14	00	}	6	8	22	00	6.	1394	373278	373641.	+363		374549.	+1271	374489.	+1211	373998.
	Perpignan - -	42	41	52	00																
	Paris and Bourges					diff.	1	45	9	19	1.	7525	106612	106658.	+ 46		106989.	+ 377	106971.	+ 359	106820.
4	Bourges - -	47	5	4	41	}	4	23	12	41	4.	3869	266666	266983.	+317		267560.	+ 894	267518.	+ 852	267178.
	Perpignan - -	42	41	52	00																
	Bourges and Rodés					diff.	2	43	51	5	2.	6309	165961	166200.	+239		166599.	+ 638	166572.	+ 611	166354.
5	Rodés - - -	44	21	13	36	}	1	39	21	36	1.	656	100705	100783.	+ 78		100961.	+ 256	100946.	+ 241	100824.
	Perpignan - -	42	41	52	00																

+ 218.3. + 208.1.

Observed Celestial Arc of the Meridian between Greenwich and Perpignan.							Meas[d] Terrest[l] Arc.	on a Sphere.								
		Latitudes.			Differences.			Semidiam.	3486975.		1st R.S.D.	{ 179.047. to 178.047	2d. R.S.D.	{ 191.483 to 191.483	3d R.S.D.	{
		°	′	″	°	′	″	Fath[s]	Fath[s]		Fath[s]		Fath[s]		Fath[s]	
1	Greenwich Rl. Ob. -	51.	28.	40.	} 0.	26.	30.7	26898	26891	− 7	27000	+102	26995	+ 97	26953	
	Tower of Dunkirk -															
2	Tower of Dunkirk -	51.	2.	9.3	} 2.	11.	55.3	133758	133811	+ 53	134299	+541	134274	+516	134073	
	Paris Rl. Ob. - -															
3	Paris Rl. Ob. - -	48.	50.	14.	} 1.	45.	11.4	106647	106696	+ 49	10 7024	+377	107006	+359	106847	
	Tower of Bourges -															
4	Tower of Bourges -	47.	5.	2.6	} 2.	44.	0.2	166115	166352	+237	166753	+638	166727	+612	166516	
	N. D. at Rodés -															
5	N. D. at Rodés -	44.	21.	2.4	} 1.	39.	6.4	100448	100526	+ 78	100704	+256	100689	+241	100567	
	St Jaumes at Perpignan	42.	41.	56.												
	Greenwich & Perpignan				8.	46.	44.	533866	534276	+410	535780	+1914	535691	+1825	534956	

The measured Arc between Paris and Dunkirk is taken a Mean between 125517½ and 125495 Toises, or 133770 and 133746 Fathoms. See
The Latitudes of Dunkirk, Bourges, Rodés, and Perpignan, are deduced from the Celestial Arcs given by M. DE LA CAILLE, in pag. 240,
Mérid. vérifiée.

24.		534909.		533880.		533801.		533734.		534928.		533652.4	
94.		27288.		27234.		27224.		27212.		27288.		27234.	
30.	+1043	507621.	+934	506646.	−41	506577.	−110	506522.	−165	507640.	+953	506418.4	−268.6
32.	+ 323	133700.	+291	133440.	+31	133398.	− 11	133354.	− 55	133705.	+296	133416.5	+ 7.5
98.	+ 720	373921.	+643	373206.	−72	373179.	− 99	373168.	−110	373935.	+657	373001.9	−276.1
20.	+ 208	106796.	+184	106590.	−22	106569.	− 43	106549.	− 63	106796.	+184	106550.4	− 61.6
78.	+ 512	267125.	+459	266616.	−50	266610.	− 56	266619	− 47	267139.	+473	266451.5	−214.5
54.	+ 393	166320.	+359	166002.	+41	165990.	+ 29	165986.	+ 25	166335.	+374	165908.5	− 52.5
24.	+ 137	100805.	+100	100614.	−91	100620.	− 85	100633.	− 72	100804	+ 99	100543.	−162.

+125.17 +112.09 −4.92 −13.2 9.8 +114.37 −32.23

Computed Terreſtrial Arc of the Meridian between Geenwich and Perpignan.

on different Ellipſoids.										on different Spheroids.			
S.D.	216.06 to 215.06	4th R. S. D	222.55 to 221.55	5th R. S. D.	230. to 229.	6th R. S. D.	310.3 to 309.3	7th R. S. D.	540. to 539	1st R. S. D.	222.55 to 221.55	2d R. S. D.	179.4 to 178.4
th'		Fath'		Fath'		Fath'		Fath		Fath'		Fath'	
53	+ 55	26946	+ 48	26893	− 5	26883	− 15	26872	− 26	26946	+ 48	26893	− 5
73	+ 315	134042	+284	133782	+24	133739	− 19	133695	− 63	134047	+289	133758	0
47	+ 200	106831	+184	106625	−22	106604	− 43	106584	− 63	106831	+184	106585	− 62
16	+ 401	166474	+359	166156	+41	166144	+ 29	166140	+ 25	166489	+374	166062	− 62
67	+ 119	100548	+100	100358	−90	100364	− 84	100377	− 71	100547	+ 99	100287	−161
56	+1090	534841	+975	533814	−52	533734	−132	533668	−195	534860	+994	533585	−281

. See Sect. VI. Art. XII.

. 240, 241, *Mém. de l'Acad.* 1758, and making the proper Reductions for the Diſtances of the Stations of the Sector at each Place, as given in the

SECTION

SECTION SEVENTH.

An Account of the obſervations made during the courſe of the trigonometrical operation for the determination of terreſtrial refraction. Plate X.

ARTICLE I. *Preamble.*

ASTRONOMICAL refraction, or that which the rays of light ſuffer in paſſing from the heavenly bodies to our earth, hath, by the inveſtigations of different philoſophers, been nearly aſcertained. From the theory of dioptrics, as well as experience, it hath been proved, that the rays, in coming from a very rare into a very denſe medium, are gradually bent downwards, out of their rectilinear direction, into lines more or leſs curved in proportion to the angular diſtance of the objects from the zenith, where obliquity ceaſing, refraction ceaſes likewiſe; ſince from that point light takes the ſhorteſt route through the refracting medium to the eye of the obſerver. Hence it follows, that the apparent altitudes of celeſtial objects are greater than they otherwiſe would be by the quantity of this refraction, which is greateſt at the horizon, amounting there to $33'$.

The late Dr. BRADLEY from his experience has ſhewn, that in the mean ſtate of the barometer taken at 29.6 inches, and of FAHRENHEIT's thermometer at 50°, the refraction at 45° of altitude is $57''$ (according to Dr. MASKELYNE only $56''\frac{1}{2}$).

In other ſtates of the atmoſphere, it varies with the height of the barometer, and inverſely with that of the thermometer augmented by the number 350 to the number 400.

It is therefore obvious, that terreſtrial objects as well as celeſtial, muſt ſuffer a refraction greater or leſs, according as they are leſs or more removed from the horizon; and that ſuppoſing celeſtial refraction to be perfectly aſcertained, the meaſure of the lower part of any of its curves, co-inciding with a particular object on the ſurface of the earth, ſhould give the quantity by which the apparent altitude of that object would exceed its real altitude, or what would be its angle of elevation, if no ſuch effect as refraction did exiſt in nature.

The inſtrument made uſe of in the triangular operation was extremely well calculated, as will be remembered from its deſcription, for meaſuring with much exactneſs ſmall angles of elevation or depreſſion, and conſequently was in that reſpect very fit for the purpoſe, if the multiplicity of other buſineſs we had on our hands at the time had permitted refraction to become a primary, inſtead of being only a ſecondary object. This will readily be conceived by thoſe who have any idea of the trouble of conducting, eſpecially at a late ſeaſon of the year, an operation of the nature of that in which we were engaged. Along with the lights on the French ſide of the Channel, we had by day as well as by night our own inland obſervations to attend to; the very circulation of orders to the men poſted at the different ſtations from twelve to fifteen or twenty miles off, in different directions around the horizon, when any part of the arrangements failed, ſo as to render a repetition of lights neceſſary, was not a matter of ſmall detail.

But beſides the important buſineſs of the triangles, which engroſſed almoſt our whole attention, it is ſufficiently obvious, that, in order to have been enabled to make concluſive obſervations, the relative heights of the ſtations ſhould in ſtrictneſs all have been aſcertained by levelling: for purpoſes of this ſort geometrical determinations, however good in other reſpects they may be, ſhould not here be admitted, becauſe they involve the very point in queſtion, that is, the height, which ſhould be obtained independently of angular meaſurement. Beſides barometers and thermometers at both ſtations, two obſervers and two inſtruments of the ſame kind would have been neceſſary, for taking at the ſame inſtant the reciprocal angle of elevation and depreſſion *.

Although, therefore, in our ſituation the circumſtances did not admit of concluſive obſervations on terreſtrial refraction, conſidered either by the mean or its extremes; nevertheleſs, ſince in a variety of caſes angles both of elevation and depreſſion were reciprocally obtained at the ſame ſtations, but at different times, it is hoped, ſuch new light will be thrown on the

* Dr. MASKELYNE, in a letter that I lately received from him, remarks, that it would be of uſe to have a perſon to note the thermometer at the object as well as at the ſtation of the obſerver, whereby (if niceties of this ſort were of conſequence) the refraction might be more accurately computed by the application of a new correction. Thus, calling $r = \frac{a}{10} = \frac{1}{10}$th of the arc of diſtance; b = the height of the uniform atmoſphere; t = the difference of the thermometers at the two ſtations; x = the difference of altitude of the two ſtations above a common level; the correction would then be $= \frac{rtb}{400x}$; and the true or whole refraction would be $= r \mp \frac{rtb}{400x}$, according as the thermometer ſtood lower or higher at the upper ſtation.

matter as may poſſibly hereafter lead to further inveſtigations of this curious, but at preſent vague and indetermined ſubject: for from theſe obſervations it will appear, that terreſtrial refraction, inſtead of being $\frac{1}{11}$th of the comprehended arc, according to M. BOUGUER, $\frac{1}{7}$th according to Dr. MASKELYNE, $\frac{1}{13}$th according to M. LAMBERT, varies from $\frac{1}{4}$ to $\frac{1}{13}$th part of that arc; and perhaps, if it had been poſſible for us to have tried it on heights conſiderably more elevated, we ſhould have found it almoſt wholly to vaniſh.

ART. II. *Relative heights.*

Before we proceed to give any account of the obſerved angles of elevation or depreſſion, at the ſtations reciprocally, for trying the quantity of terreſtrial refraction, it will be proper to call to remembrance, that, in the meaſurement of the baſe on Hounſlow Heath, the mouth of the pipe at Hampton Poor-houſe was ſhewn to be elevated about 60 feet above low-water ſpring tides at the ſea, as far as could then be determined by referring it to the ſurface of high water at Iſleworth; and that the extremity of the baſe near King's Arbour was found by levelling to be higher than the former end by 31 feet 3 inches.

The mouth of the pipe at the ſouth-eaſt end of the baſe of verification at High Nook near Dymchurch, in Romney Marſh, Lieut. FIDDES found by levelling to be above low-water mark at ſpring tides 22.1 feet.

The top of the parapet of the north turret of the Keep of Dover Caſtle was found by Lieut. HAY, of the Royal Engineers (by levelling from the top of the cliff at Queen Elizabeth's gun downwards, and adding to that the height of the

ground and Caſtle above the ſaid gun) to be 465.8 * feet above low water at ſpring tides. Having alſo meaſured a baſe for the purpoſe, he determined the height of the cliff geometrically, which agreed within leſs than a foot of the reſult by levelling. In juſtice to this very meritorious Officer it is incumbent on me to ſay, that not only on this occaſion, but on every other during the progreſs of our operations near Dover, his aſſiſtance was moſt eſſentially uſeful.

The height of the ball of St. Paul's above the Thames at Paul's Wharf, and the height of Shooter's Hill Inn above the Gun Wharf in Woolwich Warren, were ſeverally determined in 1773, at which time the experiments were carrying on for the purpoſe of finding a theory for the meaſurement of altitudes by the barometer.

The height of Severndroog Caſtle, lately built on Shooter's Hill, has ſince been deduced from that of the Inn.

Laſtly, the altitudes of all the intermediate ſtations, as expreſſed in the three colums towards the right hand of the general table of reſults, placed at the end of the preceding ſection, have been eſtabliſhed by the reciprocal angles of elevation or depreſſion, gradually carried on from ſtation to ſtation, throughout the whole ſeries of triangles, whereby the two extremities are connected together; and no greater uncertainty has been found at Hampton Poor-houſe than a few feet, occaſioned no doubt by the uncertainty of terreſtrial refraction: for it is to be remarked, that, to the weſtward of Greenwich, no double but only ſingle obſervations were obtained; wherefore, the

* Sir THOMAS HYDE PAGE, when engineer at Dover, at my requeſt, had been ſo obliging as to order his workmen (he himſelf being ill at the time) to determine the height of the turret of the Keep, which, by a miſtake of about nine feet in the height of the cliff, they made 475 feet above low-water mark.

rela-

relative heights of these stations have been determined by taking $\frac{1}{13}$th of the arc of distance for the effect of terrestrial refraction.

Art. III. *General Theorem.*

Let C (Plate X. fig. 11.) be the center of the earth considered as a sphere; Ss the surface; H*h* two places at the same height above the surface; HO the horizontal line, or apparent level at the place H; and *ho* the horizontal line, or apparent level at the place *h*; also, let C*m* bisect the angle at C.

Then, because the angles *m*HC and *mn*H are right ones, the angle *m*H*n*, or *mhn*, is equal to the angle *m*CH or *m*C*h*; that is to say, if two places H, *h*, are of equal heights, the one as seen from the other is depressed below the horizontal line of the place of observation, by an angle equal to half of the arc of the great circle contained between them, or half the angle at C. Hence it follows, that any distant object is higher or lower than the place of observation, according as the depression is less or greater than half the contained arc, supposing no such effect as refraction to exist in nature.

Art. IV. *Determination of the refraction between Dover Castle and Folkstone Turnpike.*

Let D (fig. 12.) be the place of the axis of the telescope on the north turret of the Keep in Dover Castle; T the ground at the station near Folkstone Turnpike; DO the horizontal line; and SL=CD.

The distance of the stations is 31554.6 feet, which, taking the obliquity of the direction into consideration, gives 61188 fathoms = 1°; and consequently 5′ 9″.4 for the length of the contained

contained arc of distance nearly, one-half of which, or $2'\ 34''.7$, is equal to the angle ODL.

At the station D, the ground at T was, by observation, elevated $8'\ 37''$ equal to the angle TDO, to which adding ODL $= 2'\ 34''.7$, we have for the angle TDL $11'\ 11''.7$.

Now, if the distance of the stations be taken as radius, the lines TO, TL, &c. will be nearly as the tangents of their opposite angles; therefore the angle TDL, with the distance 31554.6 give TL $= 102.7$ feet, or what the ground at the station T would have been higher than the axis of the telescope at D, if there had been no refraction.

But the axis of the telescope, when at the station T, was 5.5 feet above the ground; therefore $102.7 + 5.5 = 108.2$ feet would be the height of the axis at T above the axis at D.

Now, let T (fig. 12.) be the place of the axis of the telescope, when the instrument stood at Folkstone Turnpike; D the top of the parapet of the north turret in the Keep of Dover Castle; TO the horizontal line; and CL $=$ ST.

At the station T, the parapet of the turret was by observation depressed $14'\ 17''.5 =$ OTD, from which subtracting half the arc of distance $2'\ 34''.7$, there remains for the angle LTD $11'\ 42''.8$. This last angle, with the former known distance, give LD $= 107.5$ feet, or what the parapet was lower than the axis at T, if there had been no refraction.

But the axis of the telescope when at D was 3.2 feet above the parapet; hence $107.5 - 3.2 = 104.3$ is what the axis at D would be lower than the axis at T.

In this case it is evident, that half the sum of 108.2 and 104.3, or 106.25 feet, is the difference of the relative heights of the axis at the two stations by a mean refraction; and that this mean refraction is subtended by half the difference, or

$\frac{108.2-104.3}{2}=1.95$ feet. Hence, as the diſtance $=31554.6$: rad :: 1.95 : tang. 12″.8 the mean refraction.

For ſuppoſe t (fig. 12.) to be the true place of the ground, then the elevation TDO − the refraction = TDt, or 8′ 37″ − 12″.8 = 8′ 24″.2 = the angle tDO; therefore tDO + ODL = 10′ 58″.9 = tDL; whence tL = 100.8 feet, to which adding 5.5 feet, the height of the axis above the ground, we have 106.3 feet, the height of the axis at T above the axis at D as before.

Alſo, if d (fig. 13.) be the true place of the parapet, we ſhall have the depreſſion + the refraction, or OTD + DTd = OTd = 14′ 30″.3, and OTd − OTL, or 14′ 30″.3 − 2′ 34″.7 = 11′ 55″,6 = the angle LTd. Hence, Ld = 109.5 feet is what the parapet of the turret would be lower than the axis at T, from which taking 3.2 feet, the height of the axis above the parapet, there remains, as before, 106.3 feet, for the difference of the heights of the two ſtations.

	Feet.
The axis of the teleſcope in Dover Caſtle being above low-water ſpring tides . .	469.
To this add the height of the axis at the turnpike above that at Dover . .	106.3
We then have, for the height of the axis at the turnpike above low water, nearly . .	575.3

And 5′ 9″.4 the contained arc of a great circle or arc of diſtance being divided by 12″.8, the mean refraction at the two ſtations, we have in this inſtance *about* $\frac{1}{14}$*th part for the quantity of terreſtrial refraction.*

ART.

ART. V. *Refraction on the diſtance between Dover Caſtle and Calais.*

Let D (fig. 14.) be the place of the axis of the teleſcope, on the north turret of the Keep in Dover Caſtle, as before; G the top of the great baluſtrade of the ſteeple of Notre Dame Church at Calais; DO the horizontal line; and CL = SD.

The diſtance of Dover from Calais by the triangles is 137450 feet, which, allowing 61169 fathoms for a degree, gives 22′ 28″.2 for the length of the contained arc nearly; half of which 11′ 14″.1 is equal to the angle ODL.

The height of D above low water at ſpring tides, as before, is . . . . 469 feet.

The height of G is . . . 140.5

Therefore the difference is . . 328.5 = GL.

Then, as 137450 : rad :: 328.5 : tang. 8′ 13″ = LDG.

To which adding the angle ODL , = 11 14.1

We have . . . . 19 27.1 for the angle ODG, or what G would be depreſſed below the place of obſervation, if there was no ſuch effect as refraction. But the depreſſion by obſervation was found to be 17′ 59″; wherefore the difference is 1′ 28″.1, by which, dividing the length of the arc of diſtance 22′. 28″.2, *we have in this inſtance* 15.3, *or between* $\frac{1}{13}$*th and* $\frac{1}{14}$*th part, for the quantity of terreſtrial refraction.*

ART. VI. *Refraction on the diſtance between Allington Knoll and Tenterden.*

Let K (fig. 15.) be the place of the axis of the teleſcope at Allington Knoll; T the top of the flag-ſtaff on Tenterden Steeple;

Steeple; SC the earth's ſurface; KO the horizontal line at right angles to KC; and SL = CK.

The diſtance between Allington Knoll and Tenterden has by the triangles been found to be 61775.3 feet, which, allowing 61234 fathoms = 1° gives 10′ 5″.3 for the length of the contained arc CS nearly. Half of this arc = 5′ 2″.6 is the angle OKL, from which ſubtracting the obſerved angle of depreſſion of T as ſeen from K = OKT = 3′ 51″, there remains the angle TKL = 1′ 11″.6, and conſequently this angle will be ſubtended by 21.4 feet = LT, or what the top of the flag-ſtaff at Tenterden would have been higher than the axis of the teleſcope at the Knoll, if there had been no refraction.

But the top of the flag-ſtaff on Tenterden ſteeple was 3.1 feet higher than the axis of the teleſcope when the inſtrument ſtood at that ſtation; therefore, 21.4 − 3.1 = 18.3 is what the axis at Tenterden would have been higher than the axis at the Knoll, if there had been no refraction.

Again, let T (fig. 16.) be the place of the axis of the teleſcope on Tenterden Steeple; K the ground at the ſtation on Allington Knoll; TO the horizontal line; and CL = ST.

At the ſtation T, the depreſſion of the ground at K, or the angle OTK, was obſerved 3′ 55″, which being ſubtracted from 5′ 2″.6 = OTL = half the arc of diſtance, there remains 1′ 27″.6 = the angle KTL. This laſt angle, with the diſtance between the ſtations, 61775.3 feet, give KL = 26.3 feet, for what the ground at K would have been higher than the axis at Tenterden, if there had been no refraction.

But the axis of the teleſcope, when at the ſtation K, was 5.5 feet above the ground; therefore, 26.3 + 5.5 = 31.8 is what the axis at the Knoll would be higher than the axis at Tenterden.

Hence it follows, that ſuppoſing the refraction to have been the ſame at each of the ſtations when the obſervations were made, half the difference of theſe heights, or $\frac{31.8-18.3}{2} = 6.7$ feet, would be the difference between the relative heights of the axis at the two ſtations; and that the quantity of refraction would be ſubtended by half the ſum, or $\frac{31.8+18.3}{2} = 25.05$ feet; therefore, to find the mean refraction, as the diſtance of the ſtations : rad. :: 25.05 feet : tang. $1' 23''\frac{1}{2}$ the mean refraction.

For ſuppoſing *t* (fig. 15.) to be the true place of the top of the flag-ſtaff, we ſhall then have the angle OKT the depreſſion + the angle TK*t* the refraction, or $3' 51'' + 1' 23''\frac{1}{2} = 5' 14''\frac{1}{2}$ = the angle OK*t*. Hence the angle OKT − the angle OKL = $5' 14''\frac{1}{2} - 5' 2''.6 = 0' 11''.9$ = the angle LK*t*. Now, this laſt angle, with the diſtance of the ſtations 61775.3, give L*t* = 3.6 feet, or what the top of the flag-ſtaff at Tenterden would be lower than the axis at the Knoll; and this being added to 3.1 feet (what the axis at Tenterden was lower than the top of the flag-ſtaff), we get, as before, 6.7 feet for the height of the axis at the Knoll above the axis at Tenterden.

In like manner, ſuppoſing *k* (fig. 16) to be the place of the ground at the Knoll, we have the ſum of the depreſſion and refraction, or OTK + KT*k* = $3' 35'' + 1' 23''\frac{1}{2} = 4' 58''\frac{1}{2}$ = OT*k*; and OTL − OT*k* = $5' 2''.6 - 4' 58''\frac{1}{2} = 0' 4''.1$ = the angle *k*TL. Hence, *kt* = 1.2 feet is the height of the ground at the Knoll above the axis at Tenterden, which being added to 5.5 feet the height of the axis at the Knoll above the ground, we have as before 6.7 feet for the difference of the heights.

Feet.

The height of the axis of the teleſcope at Allington Knoll above low-water mark at ſpring tides, as determined by the obſervations there, and at the ſtation of High Nook, is 329

The axis on Tenterden Steeple has been ſhewn to be lower than the Knoll 6.7

Therefore, the axis on Tenterden Steeple is higher than low water 322.3

The arc of diſtance of the two ſtations = 10′ 5″.3 being divided by 1′ 23″¼ the mean refraction, *we have in this caſe* 7″¼, *or between* ¼*th and* ¼*th part for terreſtrial refraction.*

The example in Art. IV. and this laſt are given at large, becauſe, if the points where the axis of the teleſcope was at the reſpective ſtations had been obſerved, in the firſt, one would have been a depreſſion, and the other an elevation; but in this both would have been depreſſed by obſervation.

Art. VII. *General Remarks.*

The three preceding examples being ſufficient to ſhew the mode that has been invariably adhered to in computing the effect of terreſtrial refraction, we have, in the following table, collected the whole of the reſults together, beginning with thoſe diſtances where it has been found the greateſt, and ending with theſe where it has been found the leaſt.

The titles at the tops of the columns reſpectively fully explain the nature of the table, which contains more double obſervations, made on a greater variety of very accurate diſtances, and with a better inſtrument for determining ſmall angles of elevation and depreſſion, than perhaps were ever obtained before.

fore. These results are not however offered as being free from error; on the contrary, if the circumstances had permitted this to become a principal object in our operation, the successive repetition of the observations for many times would, no doubt, have furnished still more satisfactory conclusions. It is hoped, nevertheless, that these, such as they are, may have their use, were it only by shewing the variableness of terrestrial refraction, to induce to the making of others, which, as has been already observed, would ultimately lead to a much more minute investigation of this curious and interesting subject.

The heights of the barometer and thermometer are inserted on the days on which the observations were made, merely to shew what was nearly the state of the atmosphere at the respective times. But we have not attempted to apply any correction on that account, because it could not be done in a satisfactory manner, and consequently could not be useful, unless the circumstances had permitted reciprocal observations to have been made at corresponding times with double sets of instruments, which in our situation was impossible.

By attending to the results in the table, it will in general be seen, that terrestrial as well as celestial refraction certainly diminishes as the heights of the stations above the sea increase; and that, at particular times at least, it is much greater than has hitherto been supposed, even to between $\frac{1}{4}$ and $\frac{1}{5}$ part of the arc of distance, instead of being only $\frac{1}{8}$th or $\frac{1}{14}$th part. Besides the instance of this extraordinary effect inserted in the table, between Allington Knoll and Ruckinge, where the distance of the stations is but small, and one of them little higher than the sea, we could have given another on a distance as well as on heights still more considerable, namely, Shooter's Hill and the ball of St. Paul's Church: for, supposing the

firſt to be 482, and the laſt 403 feet, above low water at the ſea, the refraction on the morning of the 1ſt of September, 1787, as obſerved at Shooter's Hill, was 1′ 47″, which is between ½d and ¼th part of the contained arc.

If the circumſtances had permitted the refraction on the diſtance between Dover and Calais to have been repeatedly tried, at the bottom of the cliff, at the top of the cliff, and again at the top of the caſtle, we ſhould probably have found the refraction at the three ſtations conſiderably different, with the ſame length of arc, or one only varying inſenſibly.

But, in order to be enabled to make concluſive obſervations of this ſort, the operation ſhould become a diſtinct one, or at moſt only comprehend ſuch others as are connected with the modifications of the atmoſphere. For purpoſes of this kind very fine levels would be requiſite; and ſome of the higheſt mountains of Scotland, ſituated near the ſea, ſuch as *Ben Nevis* and *Cruachan Ben*, where the relative heights of the ſtations might be accurately aſcertained by levelling, would ſeem to be eligible ſituations.

TABLE containing the RESULTS of the OBSERVATION

Dates of the Obſervations.	Places.	Bar.	Therm.	Stations.
		In.	°	
1787, Oct. 21.	Allington Knoll -	29.61	56	Allington Knoll and Ruckin
23.	Ruckinge - -	29.82	51½	
19.	Dymchurch Inn -	29.9	55½	High Nook and Lydd
21.	Allington Knoll -	29.61	56	Allington Knoll and High N
19.	Dymchurch Inn -	29.9	55½	
21.	Allington Knoll -	29.61	56	Allington Knoll and Tenter
26.	Tenterden Inn -	29.54	56½	
7.	Padleſworth - -	29.6	70	Padleſworth and Lydd —
1788, Aug. 18.	Frant Inn - -	29.36	58	Frant and Botley Hill —
23.	Botley Hill - -	28.89	62½	
1787, Sept. 28.	Dover Caſtle -	29.62	58½	Dover Caſtle and Padleſwort
Oct. 7.	Padleſworth - -	29 6	70	
13.	Fairlight Down -	28.81	55½	Fairlight Down and Tenter
26.	Tenterden Inn -	29.54	56½	
26.	Tenterden Inn -	29.54	56½	Tenterden and Lydd —
1788, Aug. 11.	Goudhurſt Churchyard	29.74	58½	Goudhurſt and Frant
18.	Frant Inn - -	29.36	58	
1787, Oct. 13.	Fairlight Down -	28.81	55½	Fairlight Down and Lydd
1788, Aug. 11.	Goudhurſt Churchyard	29.74	58½	Goudhurſt and Tenterden
1787, Oct. 26.	Tenterden Inn -	29.54	56½	
21.	Allington Knoll -	29.61	56	Allington Knoll and Lydd
Sept. 28.	Dover Caſtle - -	29.62	58½	Dover Caſtle and Folkſton
1788, Sept. 2.	Folkſtone Turnpike	29.55	64	

RESULTS of SINGLE OBSERVATIONS depending on the Heights of I
Coaſt of

1788, Sept. 2.	—	—	—	29.55	64	Folkſtone Turnpike and N lais
1787, Sept. 28.	—	—	—	29.62	58½	Dover Caſtle and N. D. Ch
1788, Sept. 2.	—	—	—	29.55	64	Folkſtone Turnpike and S bert —

[To face page 246.]

ATIONS for the EFFECT of TERRESTRIAL REFRACTION.

ions.		Height of the Telescope above the Sea in Feet.	Distance of the Station in Fathoms.	Contained Arc nearly	Mean Refraction.	Proportionable Part.
				′ ″	′ ″	
Ruckinge — —	A.	329.	2675.4	2 38	1 8	$\frac{1}{2}$ and $\frac{1}{3}$
	R.	37.4				
d — —	H.	27.6	5227.1	5 8	0 55	$\frac{1}{4}$ and $\frac{1}{6}$
	L.	130.4				
High Nook —	A.	329.	3864.1	3 48	0 38	$\frac{1}{6}$
	H.	27.6				
Tenterden — —	A.	329.	10296.	10 5	1 23½	$\frac{1}{7}$ and $\frac{1}{8}$
	T.	322.3				
ld — —	P.	624.	13255.5	13 2	1 31	$\frac{1}{9}$
	L.	130.4				
l — — —	F.	659.	15060.7	14 48	2 1	$\frac{1}{8}$
	B.	880.				
lesworth —	D.	469.	7093.5	6 57	0 42	$\frac{1}{10}$
	P.	642.				
Tenterden —	F.	599.	11939.1	11 46	1 12	$\frac{1}{10}$
	T.	322.3				
— —	T.	322.3	11027.8	10 50	0 55½	$\frac{1}{11}$ and $\frac{1}{12}$
	L.	130.4				
— —	G.	497.	7398.3	7 15	0 38½	$\frac{1}{11}$ and $\frac{1}{13}$
	F.	659.				
Lydd — —	F.	599.	11948.3	11 43	0 55	$\frac{1}{12}$
	L.	130.4				
terden — —	G.	497.	9062.4	8 53	0 35	$\frac{1}{13}$
	T.	322.3				
Lydd — —	A.	329.	7975.	7 51	0 21	$\frac{1}{24}$
	L.	130.4				
olkstone Turnpike	D.	469.	5259.1	5 9.4	0 12.8	$\frac{1}{24}$
	F.	575.3				

of Dover Castle and Folkstone Turnpike, combined with those on the st of France.

and N. D. Church at Ca-	F.	575.3	26697.	26 4	3 54.8	$\frac{1}{6}$ and $\frac{1}{7}$
	C.	140.5				
D. Church at Calais	D.	469.	22908.	22 28.2	0 15.4	$\frac{1}{12}$ and $\frac{1}{16}$
	C.	140.5				
and Station at Montlam-	F.	575.3	28967.	28 29	1 45.	$\frac{1}{17}$
— —	M.	639.5				

SECTION

SECTION EIGHTH.

Secondary triangles, ſubdivided into two ſets, for the improvement of the maps of the country, and the plan of the City of London and its Environs. Plate XI.

IN the ſeries of great triangles, whereby the diſtance between the meridians of the Royal Obſervatories of Greenwich and Paris has been determined, the ſame excellent inſtrument having been placed at every ſtation on our ſide of the Channel, and all the angles obſerved with the utmoſt care, it hath conſequently followed, that the baſe on Hounſlow Heath, and that in Romney Marſh, reciprocally meaſure each other within a few inches of the truth, which is an inſtance of ſuch exactneſs as probably never occurred in any former operation of this ſort. The extreme ſmallneſs of the error on the ſum of the three angles of each triangle ſufficiently proves that the general reſult would not have differed greatly, if only two of the angles had actually been obſerved. But in an operation of ſo much importance, this could not have been depended upon; nothing was to be left doubtful; and therefore, in the execution of the various parts, the moſt minute attention was paid to every circumſtance whereby the accuracy might be affected, and particularly to the placing of the lights and inſtrument reciprocally over the ſame point marking the ſtation, that no poſſible error might ariſe from parallax or excentricity.

From

From this mode of conducting the operation, it will readily be ſeen, that, if time had permitted, the ſituation of a multitude of other points in the country might have been very accurately determined, beſides thoſe actually marking the points of the triangles, whereby the ordinary maps would have been greatly improved by ſuch as choſe at any time hereafter to make uſe of theſe as ſo many given diſtances. But the circumſtances not having permitted us to multiply thoſe points to the extent that might have been wiſhed, and that would have been eaſily practicable, if the operation had commenced at an earlier ſeaſon of the year; we have therefore been obliged to limit the number to a few of the moſt conſpicuous and beſt defined objects.

Theſe ſecondary triangles are ſubdivided into two ſets. The firſt ſet conſiſts of thirty-five, whereby the relative diſtances of ſo many points have been determined from certain ſtations of the principal ſeries, beginning with thoſe objects that have been interſected from the moſt weſterly ſtations, and ſo on, proceeding gradually with the others towards the eaſt. Two angles only of each of thoſe triangles being obſerved, the third is that at the interſected object, or the ſupplement to 180°. Although the diſtances thus obtained cannot be quite ſo accurate as the ſides of the principal ſeries; yet there is no reaſon to apprehend, that they will be found to differ widely from the truth, when they come to be proved in the courſe of any ſubſequent operation, by which alone they can be put to the teſt.

Com-

Computation of the first set of secondary triangles.

N°	Triangles.	Angles.	Distances of the stations from the intersected object in feet.
		° ′ ″	
1.	King's Arbour	8 52 57	from Stanwell 10927
	St. Ann's Hill	4 4 44	from Stanwell 23720
	Stanwell Church	167 2 19	
2.	King's Arbour	28 35 34	from Harrow on the Hill 42944
	Hanger-hill Tower	89 23 52	from Harrow on the Hill 20553.3
	Harrow on the Hill	62 0 34	
3.	King's Arbour	70 1 47	from Banstead Church 80994
	Hanger-hill Tower	82 19 25.1	from Banstead Church 76807.5
	Banstead Church	27 38 47.9	
4.	Hampton Poor-house	88 58 23	from Kew Pagoda 22849
	King's Arbour	40 14 25	from Kew Pagoda 35364.5
	Kew Pagoda	50 47 12	
5. †	Harrow on the Hill	69 43 8	from Spring Grove House 35851
	St. Paul's Church	35 58 9	from Spring Grove House 57253.9
	Spring Grove House, *Sir* Jo. Banks *	74 18 43	
6.	Hanger hill Tower	19 33 4.3	from Richmond Royal Observatory 20164.4
	Spring Grove House *	82 46 15.9	from Richmond Royal Observatory 6802.1
	Richmond Royal Observatory	77 40 39.8	Hanger Hill from Spring Grove 19857 8

† The Royal Observatory in Richmond lower Park could not be seen from any of the stations of the great series of triangles, except Hanger-hill Tower, from whence the bearing of it was taken. In order to intersect this bearing, the assistance of certain operations made with the astronomical quadrant in 1783 at Spring Grove House has been called in, by the help of which the situations of the Observatory and of Spring Grove House have been determined. In like manner, the bearings of Battersea and Stretham, taken from Hundred Acres, have been intersected with the quadrant from St. Paul's and Fulham. The stations where the quadrant was used are distinguished with asterisks.

N°	Triangles.	Angles.			Distances of the stations from the intersected object in feet.	
		°	′	″		
7.	Hundred Acres	14	13	27	from Batterſea Church	50664.5
	St. Paul's *	34	3	49.2		22226
	Batterſea Church	131	42	43.8		
8.	Hundred Acres	27	51	55.6	from Stretham Church	35957.3
	Fulham Church *	46	12	54.4		23279.3
	Stretham Church	105	55	10		
9.	Hundred Acres	36	59	35.8	from Clapham Common	43351.7
	Severndroog Caſtle	33	28	20.5		47296.4
	Clapham Common, Mr. CAVENDISH	109	32	3.7		
10.	Norwood	76	19	14.5	from Argyll Street Obſervatory	40083.2
	Severndroog Caſtle	52	41	37		39963
	Argyll Street Ob. Maj. Gen. Roy	50	59	8.5		
11.	Norwood	62	30	23.5	from St. Paul's Church	37840.9
	Severndroog Caſtle	57	8	8.5		39963
	St. Paul's Church	60	21	28		
	N. B. By combining the reſults of theſe two laſt triangles a third is formed, which gives for the diſtance of Argyll Street from St. Paul's					9632
12.	Norwood	36	36	32	from Bromley College	22695.4
	Severndroog Caſtle	32	52	48		24930.6
	Bromley College	110	30	40		
13.	Norwood	31	53	3	from Chiſlehurſt Church	35777.9
	Severndroog Caſtle	67	48	12.5		20981.1
	Chiſlehurſt Church	80	18	44.5		
14.	Greenwich Royal Obſervatory	92	38	13.5	from Wanſtead Houſe	34413.6
	Severndroog Caſtle	64	46	33.5		37999.7
	Weſt Pediment of Wanſtead Ho	22	85	13		
15.	Greenwich Royal Obſervatory	131	45	43	from Loampit Hill	6352.6
	Severndroog Caſtle	14	7	0		19428.4
	Loampit Hill	34	7	17		
16.	Greenwich Royal Obſervatory	85	49	9	from Beckenham Church	25622
	Severndroog Caſtle	63	29	48		28555
	Beckenham Church	30	41	3		

N°

N°	Triangles.	Angles.			Distances of the ſtations from the interſected object in feet.	
		°	′	″		
17.	Greenwich Royal Obſervatory	22	41	33	from Eltham Church	15531
	Severndroog Caſtle	87	18	31.5		5998.3
	Eltham Church	69	59	55.5		
18.	Severndroog Caſtle	21	56	44	from Knockholt Beeches	58933
	Botley Hill	54	48	27		26951
	Knockholt Beeches	103	14	49		
19.	Severndroog Caſtle	81	40	29.4	from Leeth Hill Tower	144761
	Botley Hill	124	53	14		92668
	Leeth Hill Tower	93	26	16.6		
20.	Botley Hill	39	17	6.5	from Firedean Tower	44780.4
	Frant Church	26	58	39		62507
	Firedean Tower	113	44	4.5		
21.	Botley Hill	19	51	19.5	from Crowborough Beacon	88977
	Frant Church	77	32	33		30949.7
	Crowborough Beacon	82	36	7.5		
22.	Botley Hill	24	22	7	from Sevenoaks Windmill	44032.4
	Wrotham Hill	28	57	42		37519.8
	Sevenoaks Windmill	126	40	11		
23.	Frant Church	46	5	9	from Wadhurſt Church	20674
	Goudhurſt Church	26	21	46.5		33538.7
	Wadhurſt Church	107	33	4.5		
24.	Goudhurſt Church	42	6	25	from Brightling Windmill	58616.3
	Fairlight Down	38	5	33		63707.4
	Brightling Windmill	99	48	2		
25.	Fairlight Down	22	40	17	from Rye Church	37598
	Lydd Church	21	23	25		39734
	Rye Church	135	56	18		
26.	Fairlight Down	19	34	30	from Dengeneſs Light-houſe	81082.7
	Dover Caſtle, north turret	13	54	24.6		115030
	Dengeneſs Light-houſe	146	31	5.4		
27.	Fairlight Down	4	12	42	from Ore Church	7605.3
	Goudhurſt Church	60	29	28		90123.2
	Ore Church	115	17	50		

N°	Triangles.	Angles.			Distances of the ſtations from the interſected object in feet.	
		°	′	″		
28.	Fairlight Down	23	32	23	from Fairlight Church	5384
	Lydd Church	1	50	43		66787.7
	Fairlight Church	154	36	54		
29.	Tenterden Church	30	42	37	from Aſhford Church	46096
	Allington Knoll	46	45	7		32319.2
	Aſhford Church	102	32	16		
30.	Lydd Church	43	34	50.5	from Ruckinge Church	41682.2
	High Nook near Dymchurch	87	40	22		8758.4
	Ruckinge Church	48	44	47.5		
31.	High Nook	83	44	33.5	from New Romney Ch.	16965.4
	Ruckinge	32	17	35.5		31566.3
	New Romney Church	63	57	51		
32.	High Nook	42	44	44.5	from Lymne Caſtle	23741.6
	Allington Knoll	70	21	48		17109.6
	Lymne Caſtle	66	53	27.5		
33.	Lydd Church	2	10	29.2	from Folkſtone Church	78946.9
	Folkſtone Turnpike	27	26	22		6501.3
	Folkſtone Church	150	23	8.8		
34.	Folkſtone Turnpike	24	35	59	from Beachborough Summer Houſe	23325.2
	Padleſworth	123	46	35.2		11681.6
	Beachborough Summer Houſe	31	37	25.8		
35.	Padleſworth	32	36	0	from Walderſhare Monument	37862.5
	Dover Caſtle	62	24	5		23081.4
	Walderſhare Monument	84	59	55		

Second ſet of ſecondary triangles.

In the Paper of 1787, ſufficient reaſons have been given for avoiding St. Paul's as a ſtation in the ſeries of great triangles. Indeed, if no other objection had exiſted, the ſmoke of the capital alone would have been found extremely inconvenient. This was experienced at Shooter's Hill, where we were

were detained a whole week, before the white lights, notwithſtanding their extraordinary brilliancy, could be ſeen at Hanger-hill Tower, or even at Argyll Street, the north-eaſt wind, which then prevailed, having brought the impenetrable maſs of ſmoke between the ſtation of the inſtrument and the points to be obſerved; and at laſt we were obliged to watch all night, till towards the morning the fires of London being extinguiſhed, the white lights could then be interſected.

It is not therefore ſurpriſing, that from the ſtations of Norwood, Greenwich, and Shooter's Hill, we ſhould only be able to fix, in a ſatisfactory manner, two points in London, namely, St. Paul's and Argyll Street. Bearings, it is true, of others were taken; but that theſe might be interſected by angles not too acute, it became neceſſary to make uſe of obſervations that had been formerly obtained at Argyll Street with my own inſtrument in its vertical poſition, and at St. Paul's with the aſtronomical quadrant. Moreover, by way of finiſhing the operation, and furniſhing ſuch part of the inhabitants of the metropolis as may be curious in matters of this ſort with a ſet of diſtances that cannot fail to be uſeful to them, two new ſtations were choſen for the great inſtrument to the northward of London, one on *Hornſey Hill*, and the other on *Primroſe Hill*. Thus, from the combined operations at theſe ſeveral places, we have been able to determine the ſituation of thirty-ſeven conſpicuous points, conſiſting chiefly of the moſt remarkable ſteeples in and near the capital.

By referring to Plate XI. which is in fact the ſkeleton, but on a very ſmall ſcale, for an improved plan of London and its environs, the relative ſituation of theſe points with regard to St. Paul's, and the four neareſt ſtations of the great ſeries, will be ſeen. Some of the principal of theſe ſecondary tri-

angles have been repreſented by dotted lines in the plan. To have expreſſed more of them in that way would only have occaſioned confuſion. Here it is to be remarked, that the diſtance of Argyll Street from St. Paul's, 9632 feet, reſulting from the 10th and 11th ſecondary triangles of the firſt ſet, becomes a baſe in the quadrilateral formed by *St. Paul's*, *Argyll Street*, *Hornſey Hill*, and *Primroſe Hill*. Hence, by the obſerved angles at theſe two laſt ſtations, and the aſſumed length of one of the unknown ſides, all the angles of the quadrilateral are computed, by which means, and the true length of one ſide given, the true lengths of all the others are readily obtained.

Computation of the ſecond ſet of ſecondary triangles.

Situations determined by the great inſtrument from Hornſey Hill and Primroſe Hill.

N°	Triangles.	Angles.	Diſtances of the ſtations from the interſected object in feet.
		° ′ ″	
1.	Hornſey Hill .	46 42 41	from St. Paul's Church 23297.1
	Primroſe Hill .	83 21 27.5	from St. Paul's Church 17072.8
	St. Paul's . .	49 56 51.5	Hornſey Hill from Primroſe Hill 17949.05
2.	Hornſey Hill .	23 8 34	Argyll Street Obſervatory 23803.4
	Primroſe Hill. .	112 49 57	Argyll Street Obſervatory 10150.7
	Argyll Street Obſervatory	44 1 29	
3.	Hornſey Hill .	23 33 59	from Hampſtead Church 17972
	Primroſe Hill .	78 23 43	from Hampſtead Church 7335.5
	Hampſtead Church .	78 2 18	
4.	Hornſey Hill .	29 11 3.5	from Mr. Duvelux's Cupola 12181.2
	Primroſe Hill .	16 44 50	from Mr. Duvelux's Cupola 7198.2
	Mr. Duvelux's *Cupola*, Hornſey Lane, Highgate	134 4 6.5	
5.	Hornſey Hill .	47 30 42	from Iſlington Church 14272.5
	Primroſe Hill .	51 42 39	from Iſlington Church 13409
	Iſlington Church .	80 46 39	

N°

With the great instrument from Hornsey Hill and Primrose Hill.

N°	Triangles.	Angles.	Distances of the stations from the intersected object in feet.
6.	Hornsey Hill .	55 28 32	from Highbury House 8867.7
	Primrose Hill .	29 28 52	14845.4
	Highbury House, Mr. AUBERT	95 2 36	
7.	Hornsey Hill .	50 52 33	from St. Luke's Church 19323
	Primrose Hill .	68 59 37	16057.5
	St. Luke's Church, Old Street	60 7 50	
8.	Hornsey Hill .	62 9 30	from St. Leonard's Church 19816
	Primrose Hill .	63 36 33.5	19560.5
	St. Leonard's Ch. Shoreditch	54 13 56.5	
9.	Hornsey Hill .	61 19 7.5	from Christ Church, Spit. 22733
	Primrose Hill .	70 33 39	21149.3
	Christ Church, Spitalfields	48 7 13.5	
10.	Hornsey Hill .	49 20 38.5	from Bow Ch. 23404
	Primrose Hill .	81 21 4.5	17959.6
	Bow Church, Cheapside	49 18 17	
11.	Hornsey Hill .	42 33 43	from St. Bride's Church 23158
	Primrose Hill .	86 44 24	15689.1
	St. Bride's Ch. Fleet Street	50 41 53	
12.	Hornsey Hill .	31 16 11	from St. George's Church 21977
	Primrose Hill .	94 11 25	11438
	St. George's Ch. Bloomsbury	54 32 24	
13.	Hornsey Hill .	29 32 8.5	from St. Giles's Church 22978.4
	Primrose Hill .	100 13 30.5	11510.4
	St. Giles's Church .	50 14 21	
14.	Hornsey Hill .	28 2 38	from St. Ann's Church 24197.1
	Primrose Hill .	106 40 20	11875.4
	St. Ann's, Soho .	45 17 2	
15.	Hornsey Hill .	59 52 55.5	from Highgate Chapel 9237.8
	Argyll Street Observatory	22 37 47.6	20766.9
	Highgate Chapel .	97 29 16.9	
16.	Primrose Hill .	20 43 50	from St. Clement's Church, 14390.2
	Argyll Street Observatory	123 0 9.4	6073.8
	St. Clement's Church .	36 16 0.6	

One angle taken with the great instrument, and the other with that in Argyll S. feet Obs.

N°

	N°	Triangles.	Angles.	Distances of the stations from the intersected object in feet.	
			° ′ ″		
One angle taken with the great instrument, and the other with that in Argyll Street Observat.	17.	Primrose Hill	17 52 31	from St. Mary's Church	14148.5
		Argyll Street	127 21 15		5463.4
		St. Mary's Ch. in the Strand	34 46 14		
	18.	Primrose Hill	7 32 8.5	from St. Martin's Church	13631.6
		Argyll Street Observatory	152 0 27		3808.8
		St. Martin's Ch. in the Fields	20 27 24.5		
	19.	Primrose Hill	3 12 59.5	from the Pantheon	10295.5
		Argyll Street Observatory	102 32 39.4		591.8
		Pantheon	74 14 21.1		
A small Hadley's sextant used in Argyll Street.	20.	Primrose Hill	5 35 34	from St. George's Church	10816.5
		Argyll Street Observatory	120 13 56		1220.1
		St. George's Ch. Hanover Sq.	54 10 30		
	21.	Primrose Hill	16 7 10	from South Audley Chapel	11359.3
		Argyll Street Observatory	103 34 59		3244.5
		South Audley Chapel	60 17 51		
A small Hadley's sextant used at St. Paul's.	22.	Hornsey Hill	38 14 6	from Newington Church	8136.4
		St. Paul's Church	16 35 7		17641
		Newington Church	125 10 47		
	23.	Hornsey Hill	20 29 59	from St. Matthew's Church	21321
		St. Paul's Church	66 7 5		8165.8
		St. Matthew's Church, Bethnal Green	92 22 56		
The astronomical quadrant used at St. Paul's in 1783.	24.	Hornsey Hill	18 22 9	from St. George's Church, Ratcliff	27045.2
		St. Paul's Church	105 32 24		8846.4
		St. George's, Ratcliff	56 5 27		
	25.	Primrose Hill	30 44 17	from St. James's Church	12562.7
		St. Paul's Church	45 39 31		8978
		St. James's Church	103 36 12		
	26.	Greenwich Royal Observat.	31 5 38	from Limehouse Church	13999.3
		St. Paul's Church	27 52 40		15402
		Limehouse Church	121 1 42		

N°

	N°	Triangles.	Angles.	Distance of the stations from the intersected object in feet.
The Astronomical quadrant used at St. Paul's in 1783.	27.	Argyll Street Observatory St. Paul's Church S.W. pinnacle of the S. tower of St. Peter's Ch. Westm.	61° 47′ 27″ 39 42 24.5 78 30 8.5	from St. Peter's Church, Westm. { 6279.5 8601.8
Greatink. and Argyll Street instruments.	28.	Norwood Argyll Street Observatory *The Monument*	18 5 5 64 9 55.7 97 44 59.3	from the Monument { 36409.7 12557.5
Both with the astronomical quadrant.	29.	Jew's Harp station Black Lane station *St. Paul's Church*	52 52 53 92 12 30 34 54 37	from St. Paul's Church { 13522.0 10790.3 from Jew's Harp to Black Lane, the base of 1783 . 7744.3
One angle with the Argyll Str. instrument and the other with the astronomical quadrant.	30.	Jew's Harp station Black Lane station *Argyll Street Observatory*	89 56 55.9 36 9 50 53 53 14.1	from ArgyllStr. { 5656.8 9586.2
	31.	Argyll Street Observatory Jew's Harp station Wind Vane *of the British Museum*	95 30 56.5 30 5 26 54 23 37.5	from the British Museum { 3488.3 6511
	32.	Argyll Street Observatory Jew's Harp station *Charlotte Street Chapel*	74 26 16.6 19 1 55.9 86 31 47.5	from Charlotte Street Chapel { 1848.1 5459.4
Both angles observed with the astronomical quadrant in 1783.	33.	Jew's Harp station Black Lane station *Portland Chapel*	85 27 45 28 53 30 65 38 45	from Portland Chapel { 4097.7 8474.2
	34.	Jew's Harp station Black Lane station *Fitzroy Chapel*	60 43 55 31 12 45 88 3 20	from Fitzroy Chapel { 4015.5 6759.6
	35.	Jew's Harp station Black Lane station *Tabernacle*	63 25 50 37 14 40 79 19 30	from the Tabernacle { 4780.8 7048.4
	36.	Jew's Harp station Black Lane station *Small-Pox Hospital*	19 45 45 56 57 45 103 16 30	from the Small-Pox Hospital { 6670.5 2690.4

Both angles observed with the aſt. quadrant in 1783.	N°	Triangles.	Angles.	Diſtances of the ſtations from the interſected object in feet.	
			° ′ ″		
	37.	Jew's Harp ſtation .	4 41 45	from St. Pancras Church	5728.1
		Black Lane ſtation .	12 58 25		2088.8
		St. Pancras Church .	162 19 50		

That theſe ſecondary triangles may be more generally uſeful to the inhabitants of London and its environs, the angles, which the 53 points comprehended in Plate XI. reſpectively form with each other at the center of the dome of St. Paul's, are collected in the annexed table, together with their ſeveral diſtances from that central point. The objects are arranged into two claſſes, according as they are ſituated to the eaſtward or weſtward of the meridian of St. Paul's. Thoſe of the firſt claſs commence at the north meridian, and proceed by the eaſt to 180°. Theſe of the ſecond commence at the ſouth meridian, and proceed, in like manner, by the weſt to 180°. From this table the total angle between any two objects being had by ſimple ſubtraction, and the diſtances from St. Paul's given, the diſtances of the objects from each other are readily obtained. Whoever, therefore, ſhould be deſirous of knowing accurately his own ſituation in this great metropolis may eaſily ſatisfy himſelf, by taking two angles from the top of his houſe, with a good HADLEY's ſextant or theodelet, between any known objects near to him, and the beſt diſpoſed for the purpoſe. By the help of theſe *data*, and a very ſimple trigonometrical computation, he will obtain what he wants; and he may even ſatisfy another curioſity which will probably occur, namely, that of putting to the teſt our original operation, by trying how nearly different triangles bring out the ſame reſult. It will readily be conceived that, for trials of this ſort, the

points whose situations have been determined by the great instrument should be chosen preferably to the others; and next to these, the objects that have been fixed by one angle, taken with the Argyll-Street instrument, as more to be relied upon, than those observed with the astronomical quadrant or sextant. Thus an excellent foundation is laid for the improvement of the plan of London and its environs, which may by these triangles be rendered more accurate than would have been possible by any other mode.

Table shewing the bearings and distances of objects situated in and near London, from the center of the Dome of St. Paul's.

	Objects.	Bearings from the north merid. eastward.			Distances in feet.
		°	′	″	
Eastward of the meridian of St. Paul's.	Newington Church	9	59	39.3	17641
	St. Luke's Church, Old Street	12	57	53.7	4262.7
	St. Leonard's Church, Shoreditch	44	57	39.8	6746.4
	The west pediment of Wanstead House	55	53	46.4	36308.4
	St. Matthew's Church, Bethnal Green	59	31	37.3	8165.8
	Christ's Church, Spitalfields	70	38	37.3	5878.4
	Bow Church, Cheapside	87	48	4.1	1078.1
	Limehouse Church	92	51	6	15462
	St. George's Church, Ratcliff Highway	98	56	56.3	8846.4
	The Monument	115	15	45.7	3114.2
	Severndroog Castle, Shooter's Hill	115	28	50.4	39963
	Transit-room of Greenwich Royal Observ.	120	43	46	25655.5
	Eltham Church	123	46	4.2	41091
	Loampit Hill	134	40	48.7	23450.1
	Station at Norwood	175	47	18.4	37841
	South Meridian of St. Paul's	180	0	0	— —

	Objects.	Bearings from the south meridian, westward.	Distances in feet.
		° ′ ″	
Westward of the meridian of St. Paul's.	Stretham Church	13 57 7.8	31793.5
	Clapham Common, Mr. CAVENDISH	26 29 56.1	24563 5
	Battersea Church	52 22 27.6	22226
	St. Peter's Ch. Westm. S W pinnacle of the S. T.	52 32 13.2	8661.8
	Fulham Church	57 39 44.6	30746 3
	Kew Pagoda	71 2 36	47577.7
	Richmond, Royal Observatory	71 42 0.1	51941.1
	St. Martin's Church, in the Strand	74 28 59.2	6748.6
	Spring Grove House, Sir Jos. BANKS, Bt.	76 9 49.3	57253.9
	St. James's Church	77 49 9.8	8978
	South Audley Chapel	81 49 42.7	12211.1
	St. Mary's, New Church in the Strand	81 57 27.7	4291.6
	St. Clement's Church	85 57 36.7	3592.4
	St. Ann's Church, Soho	86 9 58.7	7753.9
	St. George's Church, Hanover Square	86 23 12.9	10304.5
	St. Bride's Church, Fleet Street	90 12 42.8	1771.7
	Argyll Street Observatory, Maj. Gen. ROY	92 14 37.7	9632
	The Pantheon	93 19 17.2	9066.8
	Hanger-hill Tower	94 24 59 6	45845.2
	St. Giles's Church	94 36 28.3	6917.3
	Portland Chapel	100 34 38.7	10301.4
	Charlotte Street Chapel	101 30 23.9	8500.4
	St. George's Church, Bloomsbury Square	103 15 20.6	6221.1
	Wind Vane of the British Museum	105 45 45.7	6701.5
	The Tabernacle, Tottenham Court Road	107 20 47	8876.2
	Fitzroy Chapel	109 41 10.2	9559.9
	Harrow on the Hill Church	112 7 58.3	58764.2
	Jew's Harp station of 1783	112 58 30.7	13522
	Primrose Hill station of 1788	123 28 40.8	17072.8
	Hampstead Church	128 56 11.8	24148.7
	St. Pancras Church	136 43 25.8	10600.7
	The Small-Pox Hospital	137 38 37.8	8732.3
	Black Lane station of 1783	147 53 7.7	10790.3
	Highgate Chapel	150 59 18.1	24062 4
	Mr. DUVELUZ's Cupola, Hornsey Lane, High.	155 27 12.8	22646
	Hornsey Hill station of 1788	173 24 32.3	23297.1
	Islington Church	174 40 21.4	9028.2
	Highbury House, Mr. AUBERT	178 43 11.6	14595.7
	Ditto, the Transit-room of his Observatory	179 1 56.6	14561.4
	North meridian of St. Paul's	180 0 0	— —

Com-

Computed latitudes and longitudes of ſome of the places in the above table.

Places.	Latitudes.	Longitude from Greenwich in degr. &c.	Longitude from Greenwich in time
	° ′ ″	° ′ ″	h. m. ſec.
St. Paul's	51 30 49.43	0 5 46.8	0 0 23.12
Highbury Houſe, Tranſit-room	51 33 12.8	0 5 50.5	0 0 23.37
St. James's Church	51 30 30.7	0 8 5	0 0 32.33
Argyll Street Obſervatory	51 30 53.05	0 8 18.36	0 0 33.224
Clapham Common, Tranſit-room	51 27 12.7	0 8 39.2	0 0 34.613
Richmond Royal Obſervatory	51 28 7.9	0 18 42.3	0 1 14.82

CONCLUSION.

IN the courſe of this Paper, an account has been given of the commencement, progreſs, and completion of an operation, the firſt of its kind in this country, undertaken by the command, and executed under the auſpices, of a moſt gracious and beneficent Sovereign, the Patron of the Sciences.

From a liberal ſupply of much better inſtruments than ever were uſed for purpoſes of this ſort on any former occaſion, and every other aſſiſtance that could contribute towards ſucceſs, the operation has undoubtedly derived ſome peculiar advantages: for, beſides a more accurate mode of meaſuring the baſes than has heretofore been practiſed, the angles of the triangles have been obſerved ſo truly, that the relative geodetical ſituations of the ſtations, as determined by plane trigonometrical computation, may be ſaid to be free from ſenſible error.

The inſtrument too, by means of its tranſit teleſcope, being admirably calculated for determining, with great preciſion, the true direction of the meridians, their convergence to each other, and conſequently the differences of longitude, have

thereby been obtained by angular measurement alone, without any regard to difference of time, more or less erroneous even with the very best time-keepers, and not perhaps to be depended upon to nearer than half a second, after taking a mean of a number of comparisons. This mode, by angular measurement, was suggested in the Paper of 1787; and we presume to think, that the result of the operation has fully verified the goodness of the method by the consistency of the pole-star observations among themselves. It may be said to be a new mode of surveying, by the help of the pole-star as a fixed point, for preserving the accuracy of the operation, successively carried on from meridian to meridian; and the same mode should be adhered to in future.

Another circumstance must likewise be noticed, as having been proposed at the same time, namely, the use of white lights for the distant stations: for without the help of these, observed with such an instrument as ours, it would have been utterly impossible to have determined accurately the distances of Montlambert and Blancnez, the first nearly forty-seven, and the last nearly forty-eight miles from Fairlight Down.

Without farther recapitulation, the Writer of this Account cannot help considering it as being incumbent on him to recommend, that the trigonometrical operation, so successfully begun, should certainly be continued, and gradually extended over the whole island. Compared with the greatness of the object, the annual expence to the publick would be a mere trifle not worthy of being mentioned. In reality, a chief part of the expence, namely, that of fine instruments, has already been incurred; and it would be a pity indeed to suffer them to be laid up and remain useless. The honour of the nation is concerned in having at least as good a map of this as there is of any

any other country. But, by proceeding with the work in the ſame manner as it has been begun, with more perfect inſtruments than have heretofore been uſed, and ſome of theſe applied in a new way, a map of the Britiſh iſlands will at length be obtained, greatly ſuperior in point of accuracy to any that is now extant.

One additional inſtrument would certainly be wanted, that is, a zenith ſector for the determination of the latitudes, when the operation came to be extended to any conſiderable diſtance from the parallel of Greenwich. But this would not be neceſſary at firſt; while ſuch a one is preparing by Mr. RAMSDEN, and which he will no doubt render the compleateſt thing of the kind, the operation ſhould be continued in the parallel of Greenwich, or in the perpendicular of its meridian, quite to the weſtern ſide of the iſland in the manner following.

In more than one place of this Paper we have had occaſion to expreſs our regret, that the recent ſeries of triangles did not afford diſtances ſufficiently great between points reciprocally viſible, for the beſt application of the pole-ſtar obſervations, to the determination of the differences of longitude. It is believed, that the obſervations themſelves are extremely near the truth, but not wholly free from error; therefore, whatever this may amount to, on double or triple the diſtance it would certainly be reduced to one-half or one-third part.

Shooter's Hill, and Nettlebed Heights on the eaſtern ſkirt of Oxfordſhire, are reciprocally viſible at the diſtance of about forty-ſix or forty-ſeven miles from each other. Nettlebed Heights, and a thin clump of trees on the Glouceſterſhire range of hills, called *Paul's Epiſtle*, about two miles weſtward from *Frog Mill*, on the left-hand ſide of the road leading from thence to Glouceſter, may likewiſe be ſeen from each other at the

the diſtance of fifty or fifty-two miles. This laſt commands a moſt extenſive proſpect over the plain of the Severn and the Welch mountains to a great diſtance beyond it. *Pen-y-Voel Hill*, called alſo the Sugar-loaf of Abergavenny, in Monmouthſhire, would become the third ſtation to the weſtward; and two, or at moſt three, ſtations more would reach St. David's Head, oppoſite to Wexford in Ireland.

But let us ſuppoſe, in the firſt place, the ſeries of triangles to be extended only to the third ſtation, in all which ſpace it would be wholly unneceſſary to obſerve any latitudes; by the pole-ſtar obſervations, repeated a ſufficient number of times on both ſides of the pole, at each of the ſtations, the length of the degree of a great circle, perpendicular to the meridian, and conſequently the differences of longitude, would thereby be obtained to the utmoſt preciſion. A determination of this ſort would abſolutely be concluſive, with regard to the length of the vertical and radius of the parallel in the latitudes of the reſpective ſtations, aſcertainable by their diſtances from the perpendicular to the meridian of Greenwich.

The ſecond part of the operation would be that of carrying a ſeries of triangles ſouthward from *Pen-y-Voel Hill*, in the direction of its meridian to the Britiſh Channel; and afterwards extending theſe triangles in the uſual manner over the whole ſouth part of the iſland between Kent and the Land's-End.

If, beſides the zenith ſector, another circular inſtrument was provided, and ſome additional annual expence allowed, in order to accompliſh more ſpeedily ſo great and uſeful a work, at the ſame time that the operations to the ſouthward were carrying on, the ſeries of triangles, in the direction of the meridian of *Pen-y-Voel Hill*, ſhould be continued to the northward throughout

out the extent of the iſland till it fell into the Murray Frith. A new meridian might then be taken more to the weſtward, perhaps that of Inverneſs, or ſome hill near it, whereby the ſeries would be extended to the North Sea, bounding the coaſts of Sutherland and Caithneſs.

It is unneceſſary here to enter into any minute detail of what ſhould be the ſucceeding parts to be carried preferably into execution, as things of this ſort would naturally preſent themſelves, in the courſe of ſuch important operations, to thoſe entruſted with the direction. It is however ſufficiently obvious, that having, as above ſuppoſed, obtained the meaſure of a portion of the meridian amounting nearly to ſixteen degrees of latitude in continuity, between the Pyrenean mountains and the northern extremity of Britain, or more than one-ſixth part of the diſtance between the equator and the pole; the things of the next conſequence to be obtained would be, the meaſures of the *radii* of the vertical and parallel in the low-lands of Scotland, that is, in the latitude of Edinburgh, and again at the northern coaſt. In each of theſe ſituations it is evident, that about three degrees of longitude might be meaſured with great exactneſs. At the north, for inſtance, *Cape Wrath* being made the central ſtation, from thence the *Orkney Iſlands* to the eaſtward and *Butt of the Iſland of Lewes* to the weſtward, being diſtinctly ſeen, would conſequently become the ſtations to the right and left.

With regard to the uſe of white lights, ſo indiſpenſably neceſſary in all operations of this ſort, no opportunities have yet offered of aſcertaining with preciſion the immenſe diſtance to which they may be ſeen in favourable circumſtances of the weather, and with ſufficient elevation of the ſtations above the ſea. Thoſe commonly uſed in the recent operation were only

three or four inches in diameter, and the largeſt but ſix or ſeven. Augmented to nine or ten inches, exhibited on the top of one high hill, and obſerved from the top of another, when there is no moonlight, and no rain or fog, they would probably be ſeen eighty or a hundred miles. In ſhort, wherever the moſt faint looming of the land in a very clear day can be diſcerned, the lights, from their extraordinary brilliancy, would undoubtedly be ſeen in a dark night, when the air was perfectly clear.

Hence, it will readily be conceived, how eaſily and accurately any trigonometrical operations that might be carrying on in England and Ireland at the ſame time might be connected with each other, by means of theſe lights, alternately exhibited and obſerved, for inſtance, on *Brath-y-pwl Point*, *Holyhead Hill*, and the *Iſle of Man*, on one ſide; and again on the *mountains of Wicklow*, *hill of Howth*, and *mountains of Mourne*, on the other.

In the Paper of 1787, and again in this, we have had occaſion to remark on the improbability of being able to determine the differences of longitude, by the inſtantaneous exploſion of light, ſo accurately as by angular meaſurement with a fine inſtrument, applied as it has been in the recent operation. But ſince, undoubtedly, there will be different opinions on this head, it will be very proper that both modes ſhould be tried, that the reſults may be compared.

To the eaſtward of Greenwich, the ſtation for exploſion might be taken at *Montlambert*, *Fienne Windmill*, or at *Folkſtone Turnpike*, in order to render the diſtance of the extreme ſtations as great as poſſible. Any of theſe points would be viſible from *Crowborough Beacon*, which would become the ſtation of the Engliſh aſtronomer with his clock and inſtruments. That of the French aſtronomer would of courſe be

taken in the moſt convenient place inland on the range of chalk hills, viſible from the place of exploſion, and the eaſieſt connected with the triangles of the meridian of Paris in the neighbourhood of *Helfaut* and *Bouvigny*. *Crowborough* is about 70 miles diſtant from *Montlambert*, and a point in the direction of theſe two near *Helfaut* would be about 32 miles inland from *Montlambert*, which would give for the extreme diſtance about 102 miles.

If experiments of the ſame kind were to be made to the weſtward of Greenwich, thoſe very ſtations, already propoſed for the continuation of the triangular operation, would be the fitteſt that could be choſen for the purpoſe.

Now, ſuppoſing the operations already mentioned in the parallel of Greenwich to be executed, the meridian of Pen-y-Voel Hill extended to the northern extremity of Scotland, and three degreees of longitude meaſured in that latitude, while the Eaſt India Company were carrying on operations of the ſame nature on the coaſt of Coromandel and in Bengal, every thing would then be done that Britain could do within her own dominions, in regard to the determination of the figure and dimenſions of the earth. If, after this, any doubts remained, theſe might eaſily be removed, by Portugal's meaſuring a degree or two of the meridian under the equator, and alſo a portion of the earth's equatorial circumference; while ſome other nation repeated the operations at the polar circle, or made new ones ſtill nearer to the pole, if ſuch ſhould be found practicable, all which has been ſuggeſted in the Paper of 1787. For the farther illuſtration of this ſubject, it will be proper to refer to fig. 3. in Plate X.

With regard to the execution of any future operation that may, and which it is hoped will, be hereafter undertaken in

Britain, there remains but one point more to be mentioned, that is, the meaſurement of new baſes. In the execution of the great map of France, no fewer than ſeveenteen were meaſured. But, with ſuch inſtruments as have been uſed in this country, a ſmaller number would ſuffice; and the beſt ſituations for the purpoſe will naturally preſent themſelves in the courſe of the operations.

Thoſe that immediately occur to the Writer of this Memoir, as likely to be found the moſt proper, are the following, *viz.*

1. On Sedgemoor in Somerſetſhire.
2. On Boſton Fens in Lincolnſhire.
3. On the ſands on the coaſt of North Wales, between Pen man Mawr and Beaumaris, . } at low water
4. On the ſands between Holy Iſland and Berwick upon Tweed, . . . } ſpring tides.
5. On Kincairden and Flanders Moſs, weſtward from Stirling.
6. On the ſands on the coaſt of Aberdeenſhire, between the mouth of the Don and Newburgh, } at low water
7. The ſands on the coaſt of Murray, between the mouths of the rivers Findhorn and Nairn, } ſpring tides.
8. On the Moan moraſs, inland from the Whiten Head, on the coaſt of Sutherland.

In the meaſurement of theſe baſes, which ſhould not be leſs than ſix, but as often as poſſible even eight or ten miles in length, there would not be any neceſſity for that wonderful exactneſs that was requiſite for determining the length of the firſt and ſecond baſes on Hounſlow Heath and Romney Marſh. Suppoſing them to be executed with the ſteel chain alone within a few feet of the truth, it would be perfectly ſufficient to ſhew

that no error of any conſequence had accumulated in carrying on the operation to theſe diſtant points reſpectively, even as far as the remote ſhore of the Northern Ocean.

Finally, in order to preſerve the primitive ſcale of diſtances, whereon the accuracy of the recent operation, and all future ones that may hereafter be connected with it, muſt always be ſuppoſed to depend, it is indiſpenſably neceſſary to eſtabliſh, without loſs of time, ſome permanent marks at the extremities of the baſe on Hounſlow Heath *. Theſe ſhould be low circular buildings, riſing but a few feet above the ſurface of the Heath, compoſed of the hardeſt materials, ſuch as granite, and conſtructed in the moſt durable manner by dove-tailing the ſtones into each other. They would reſemble thoſe baſements of ancient croſſes we often meet with, formed into regular ſteps, whereby the aſcent is rendered eaſy to the top of a circular table or platform, of ſufficient dimenſions for the reception of the great inſtrument on any future occaſion.

In the interior part of theſe little buildings, metal tablets would be inſerted, containing the name of that much-beloved Monarch in whoſe reign the operation was begun, and theſe buildings executed; the diſtance from one to the other; the angle of the baſe with the meridian; and alſo the magnetical variation.

It is not to be doubted, that the reſpective lords of the manors will readily veſt in the Royal Society, the property of the two ſmall ſpots of the Heath ſufficient for the erection of theſe

* Soon after the meaſurement of the baſe, Mr. MYLNE, F. R. S. at my deſire, was ſo obliging as to give a deſign for a building of this kind, which, being conſtructed nearly in the way of the Eddyſtone Light-houſe, executed by the ingenious Mr. SMEATON, would anſwer very well.

termini.

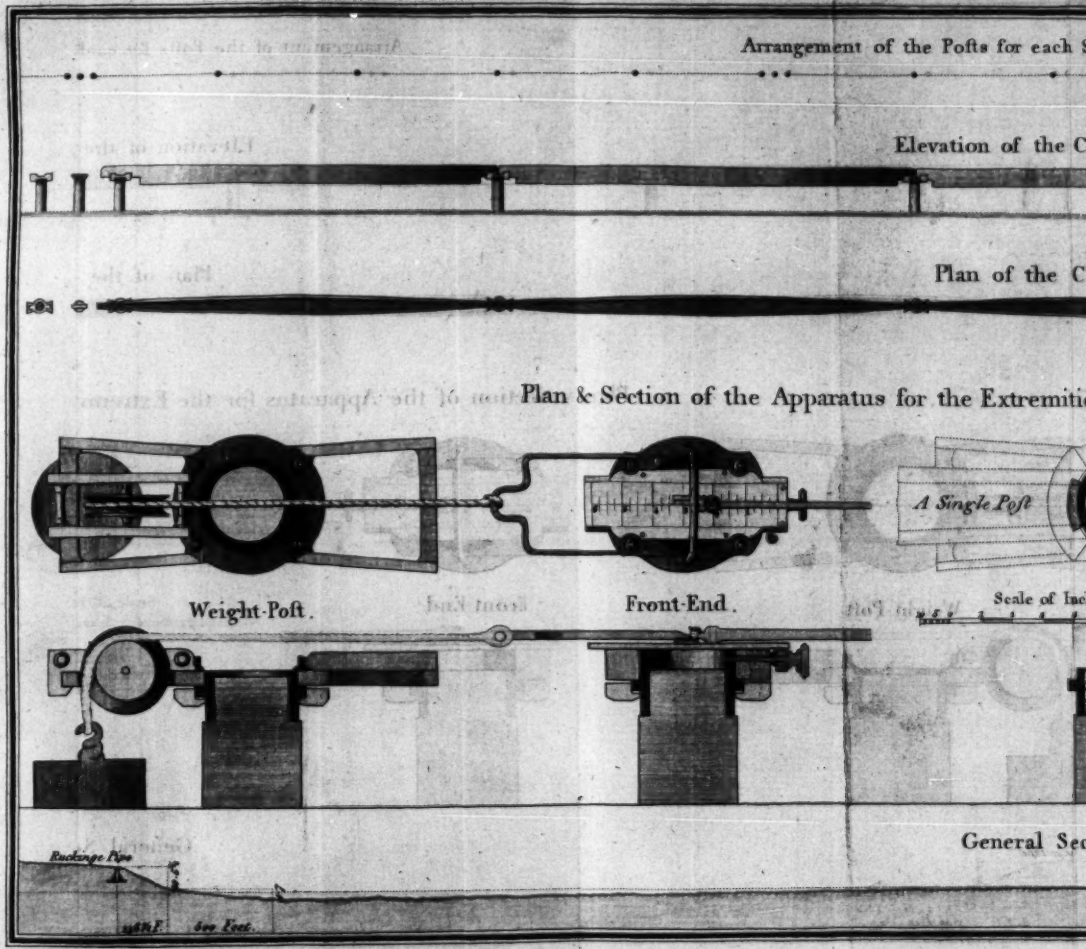
termini. They ſhould be carried into immediate execution; for, if this buſineſs ſhould be poſtponed for any length of time, there will be danger of its being altogether forgotten. In a few years the wooden pipes ſunk in the earth will decay; and thus the primitive ſcale of diſtances, which coſt ſo much labour and expence to obtain, will be irretrievably loſt.

Errata in Phil. Trans. Vol. LXXVII.

Page.	Line.	
193.	6.	from the bottom, *for* 133417 *read* 133409
194.	2.	from the bottom, *for* p. 53. and 56. of the first part *read* p. lxiii. Part Third
196.	8.	*for* 351. *read* 358.6
	10.	*read* 133417
198.	3.	from the bottom, *dele (St. Jaumes)*
199.		*dele* from the semicolon in line 4. to the end of the paragraph
202.	12.	*for* 609 *read* 521
206.	3.	*for* 27350 *read* 27342
	6.	*for* 2078 *read* 1819
207.	9.	from the bottom, *for* 27331 *read* 27337—L. 8. *for* 1758 *read* 1734—L. 3. *for* 27301 *read* 27294—L. 2. *for* 1288 *read* 1043
208.	8.	*for* 27294 *read* 27288
	9.	*for* 1177 *read* 934
	16.	*for* 27241 *read* 27243
	17.	*for* exceeds the truth 202 *read* falls short of the truth 41
	5.	from the bottom, *for* 27230 *read* 27224
	4.	——— *for* exceeds the truth 131 *read* falls short of the truth 110
209.	3.	*for* 27206 *read* 27212
	5.	*for* exceeds the truth by 46 *read* falls short of the truth by 165
210.	9.	*for* 27295 *read* 27288
	10.	*for* 1196 *read* 953
211.	5.	*for* 27243 *read* 27234
218.		*dele* from the period in L. 3. from the bottom, to the end of the page.

The above *errata* have been given, in order to free, as far as is now possible, the Paper of 1787 from the errors indicated in the Note at the beginning of Section VI. of the present Memoir, p. 201.; but that Paper cannot be rendered so perfect as it ought to have been, unless some sheets of it are re-printed.

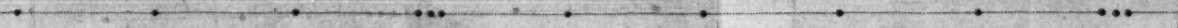
Pl.1. *FOR THE ACCOUNT of the MEASUREMENT of the* BASE of VERIFICATION in RO



ROMNEY MARSH with the *STEEL CHAIN*, *in the Autumn of 1787.*

Philos. Trans. Vol. LXXX. Tab V. p. 272

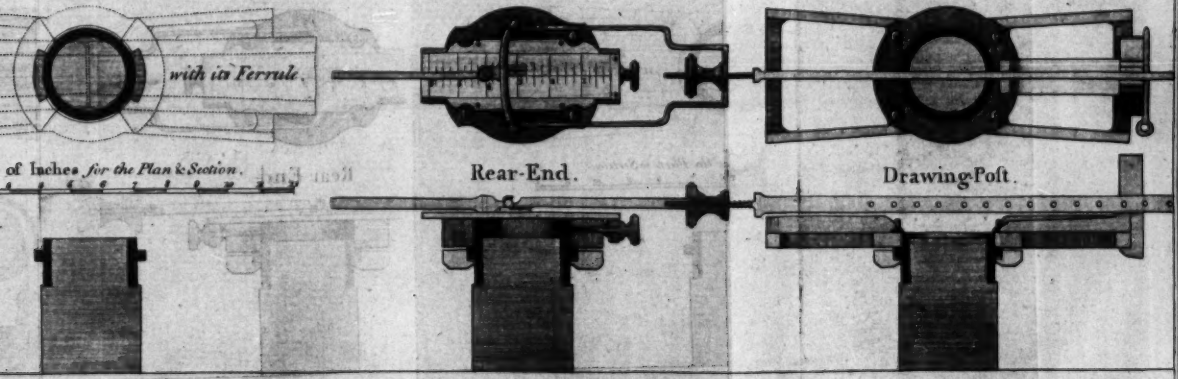
each Space of 100 Yards, or Length of 3 Chains.



the Coffering for each Chain.

he Coffering for each Chain.

emities of the Chain; *Scale 1/4th part of the real dimensions.*



l Section of the Baſe.

Basire Sc.

PLAN shewing the situation of

of the BASE of VERIFICATION measured in ROMNEY MARSH in the Autumn

Scale of Ten Thousand Feet

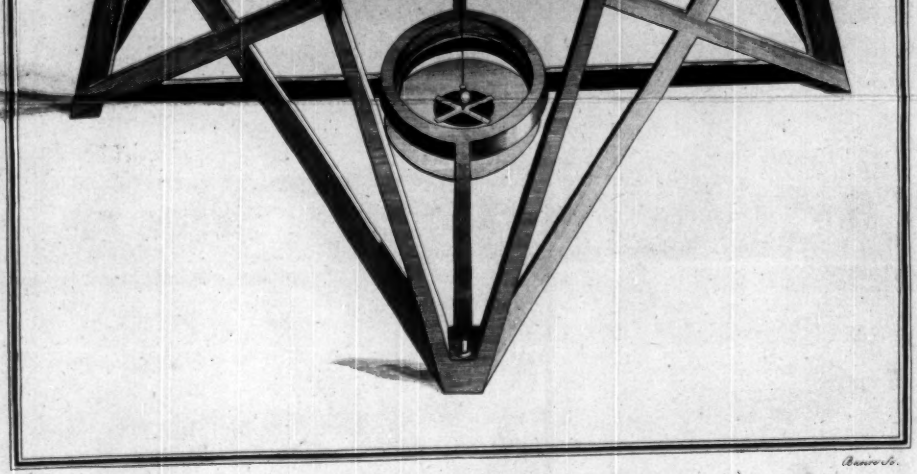
ving the situation of the BASE of VERIFICATION measured in ROMNEY MARS

Scale of Ten Thousand Feet

1 2 3 4 5 6 7 8

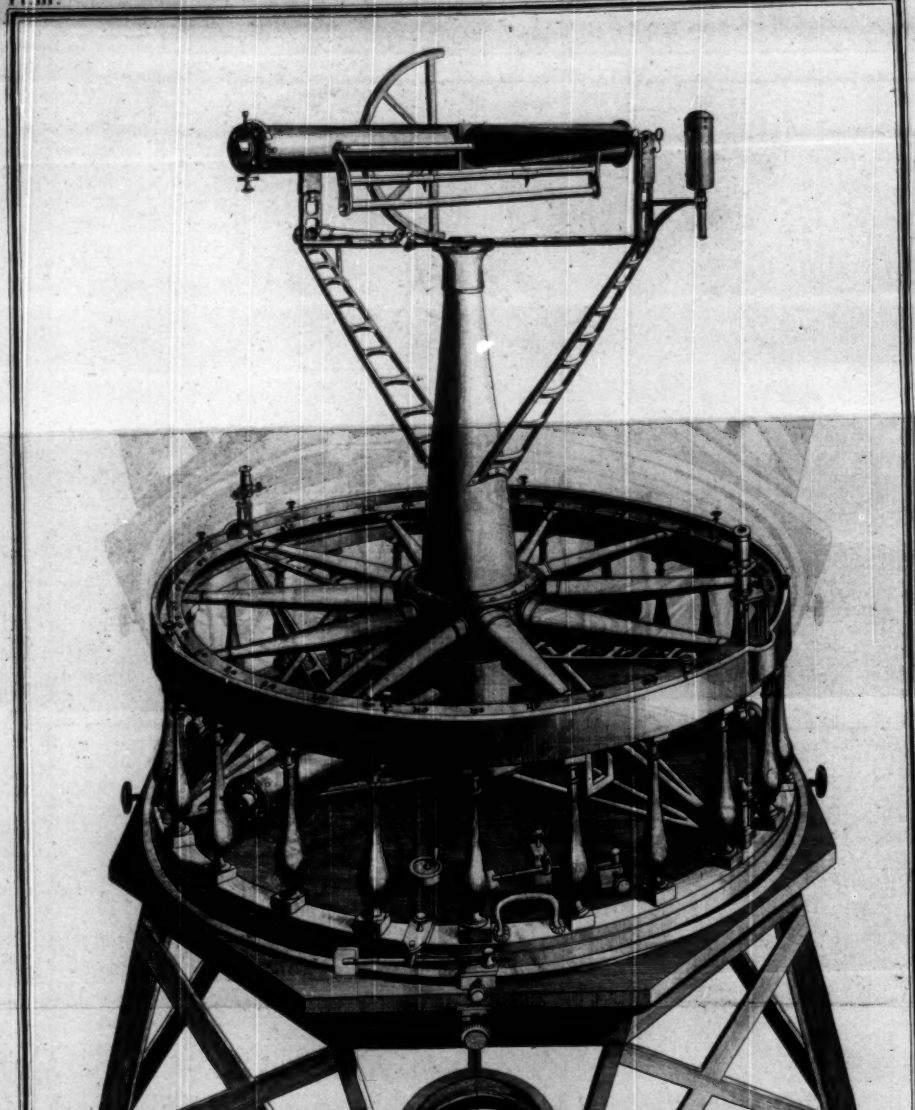
ARSH in the Autumn of 1787.

Philos. Trans. Vol. LXXX. Tab. VI. p. 272.



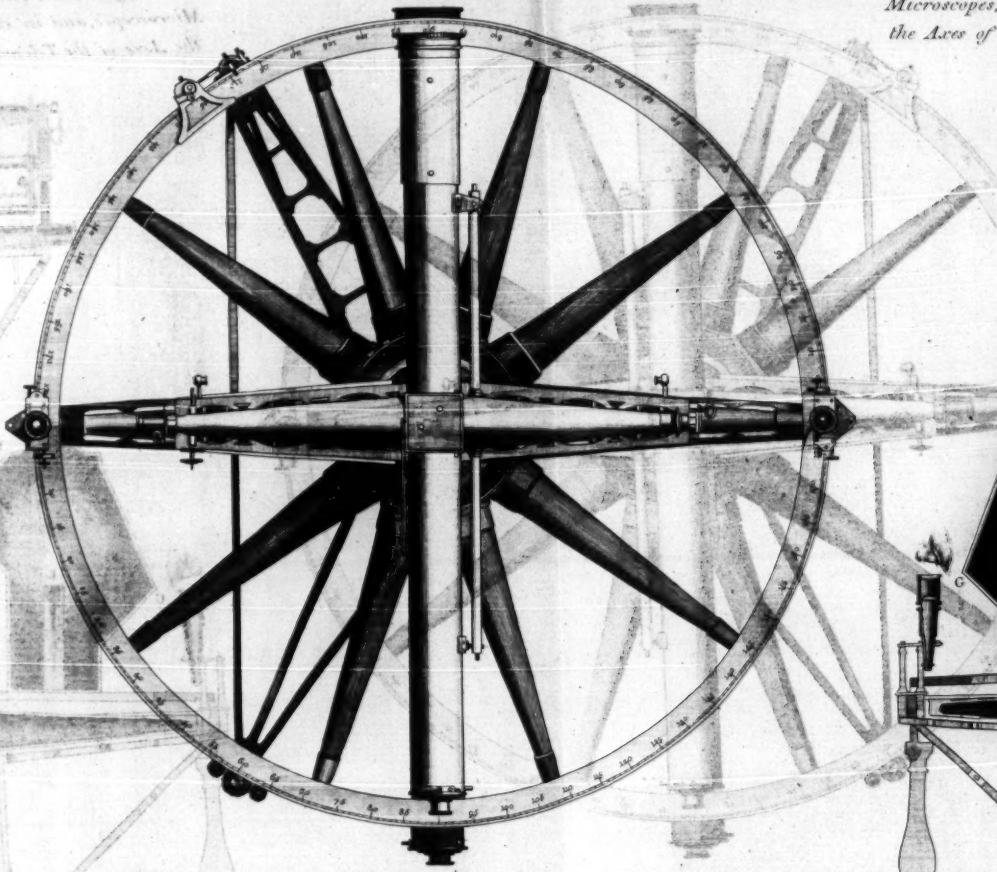
T. Milne del. Basire Sc.

General View of the Instrument.

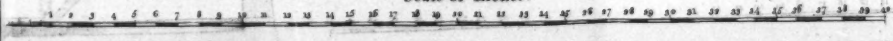


Plan *of the Instrument.*

Section *of t*
Microscopes,
the Axes of

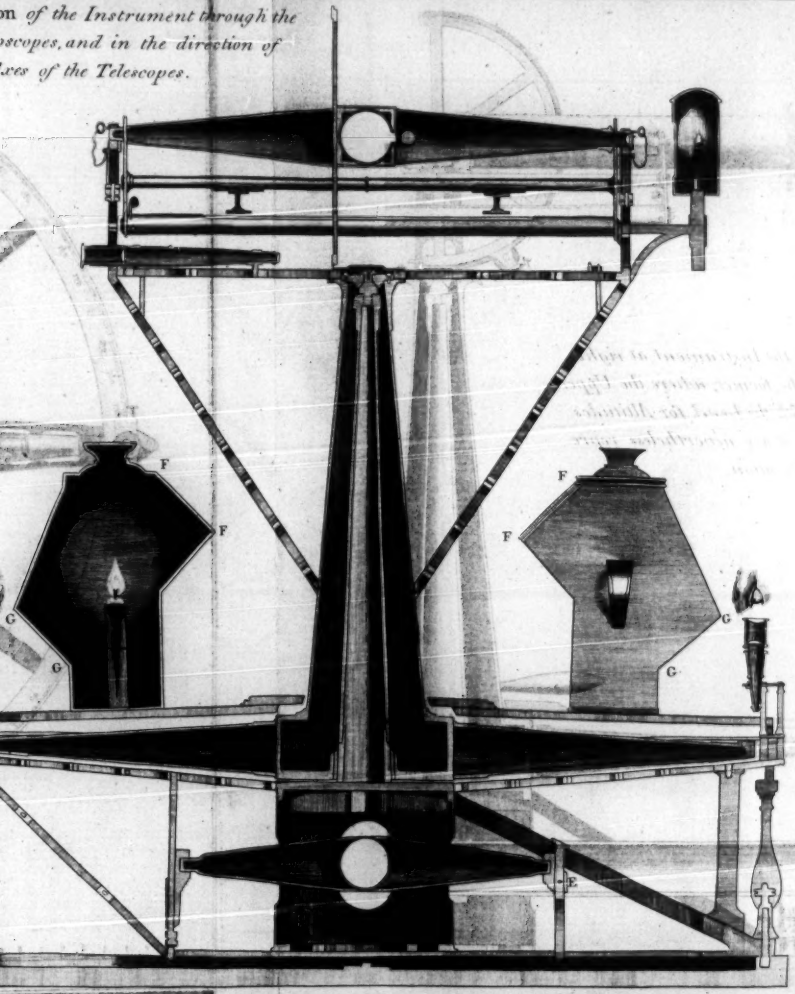


Scale of Inches.

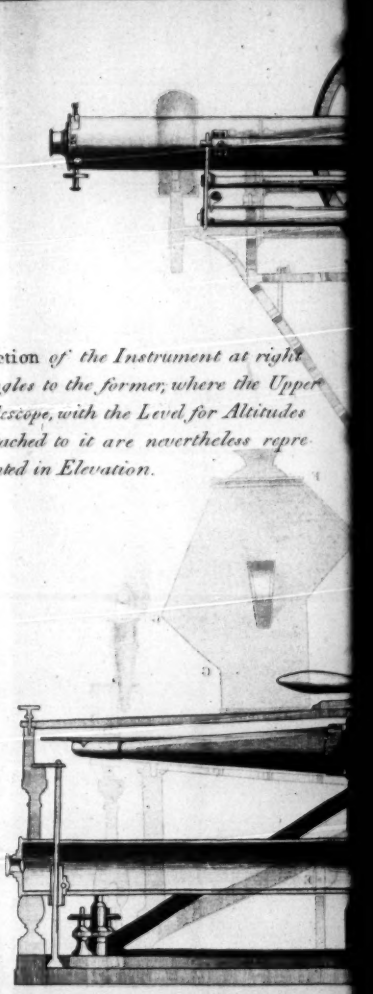


T. Milne del.

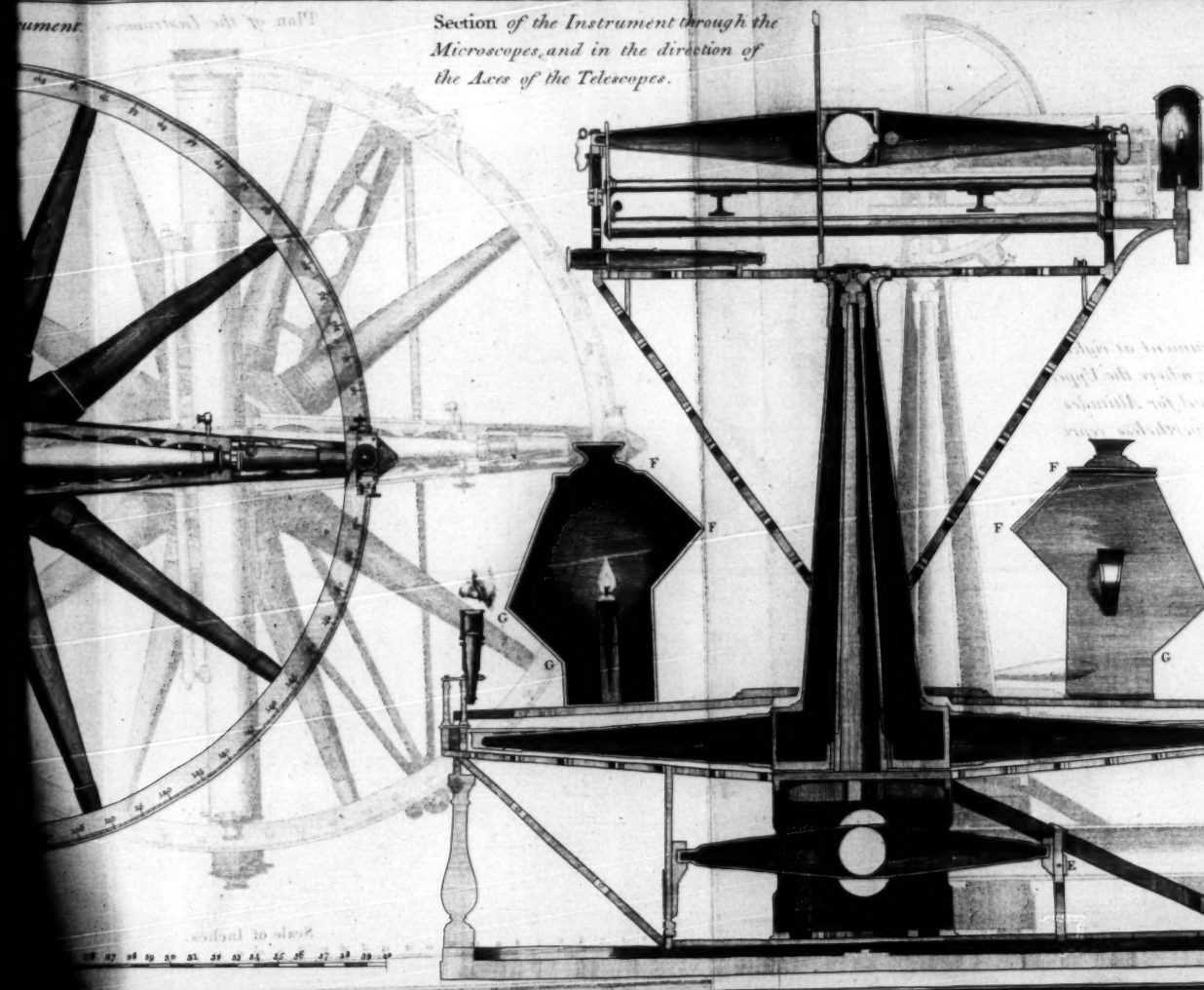
on *of the Instrument through the*
scopes, and in the direction of
xes of the Telescopes.

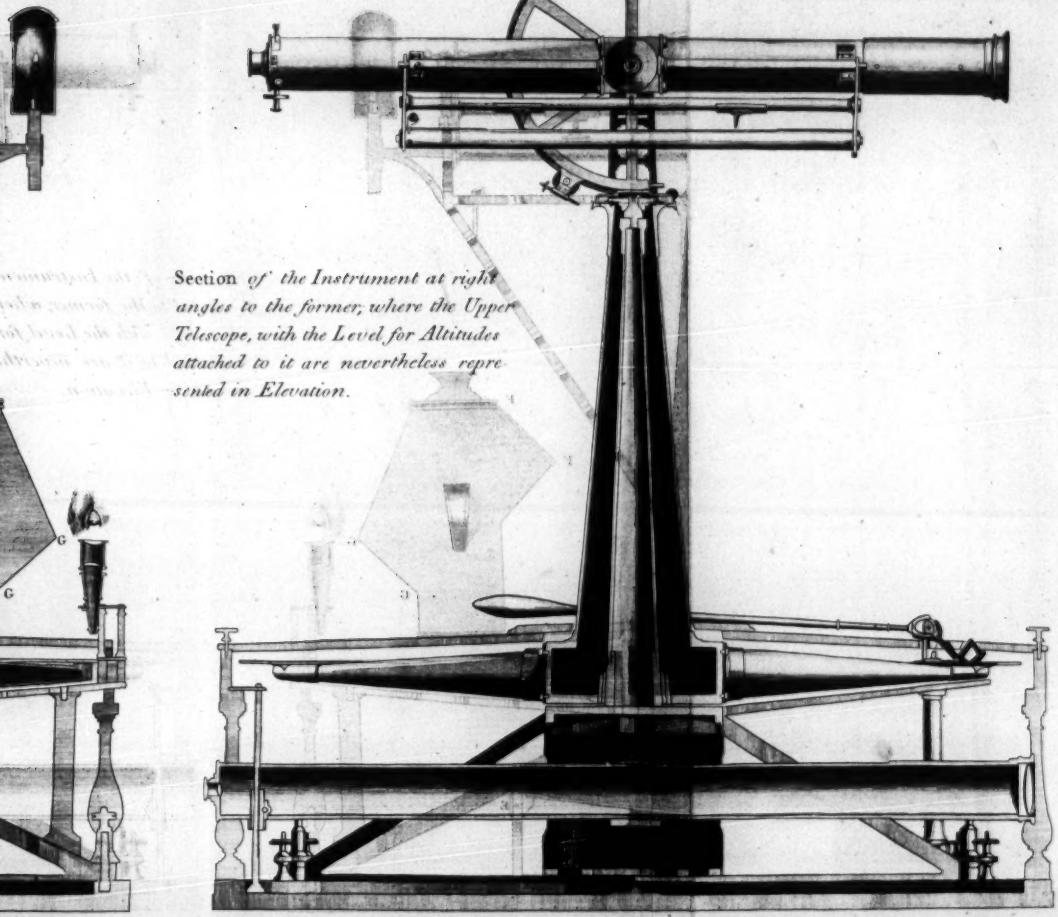


Section *of the Instrument at right angles to the former, where the Upper Telescope, with the Level for Altitudes attached to it are nevertheless represented in Elevation.*



Section *of the Instrument through the Microscopes, and in the direction of the Axes of the Telescopes.*

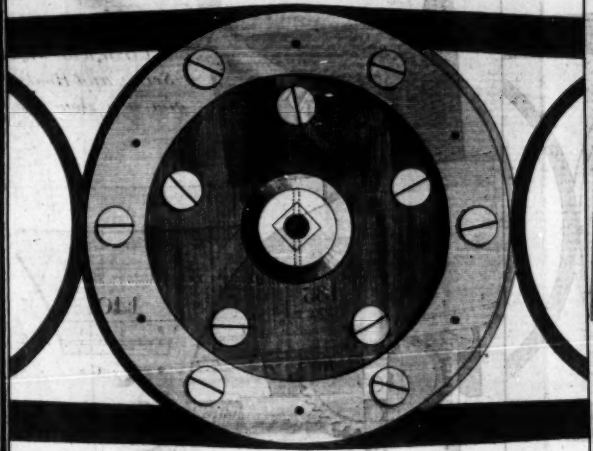




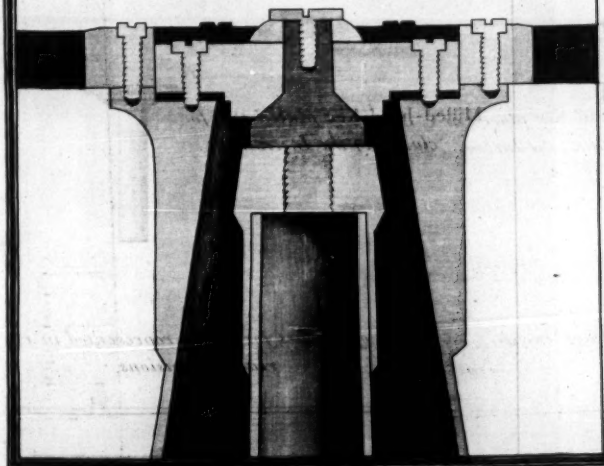
Section *of the Instrument at right angles to the former, where the Upper Telescope, with the Level for Altitudes attached to it are nevertheless represented in Elevation.*

Various parts

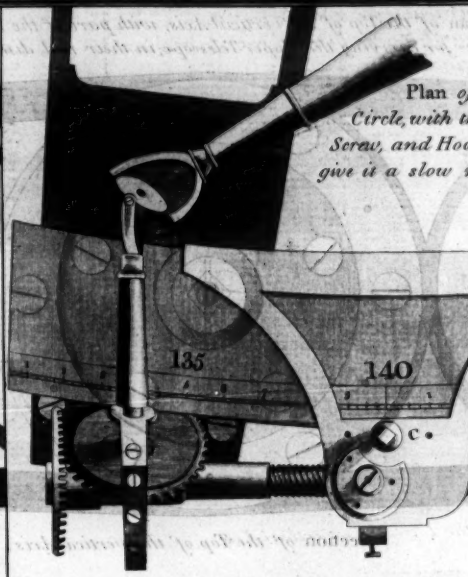
Plan *of the Top of the vertical Axis, with part of the horizontal Bar for carrying the Upper Telescope, in their real dimensions.*



Section *of the Top of the vertical Axis.*

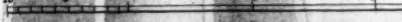


Plan *o*
Circle, with t
Screw, and Hoo
give it a slow



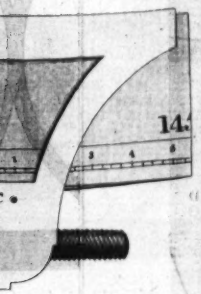
Milled-head-Key *placed at* C *for clamping the Instrument.*

Scale *of Inches for parts represented in real dimensions.*



arts of the Instrument represented to large Scales.

Plan *of a part of the*
with the Clamp, Wheels,
nd Hooks-joint, that
low motion.



nted in their

Plan *of one of the Feet of the Instrument.*
Scale ½ of the real dimensions.

Section *through one of the Feet-Screws,*
whereby the Instrument is levelled; and of
the three Mahogany Planes that are under it.

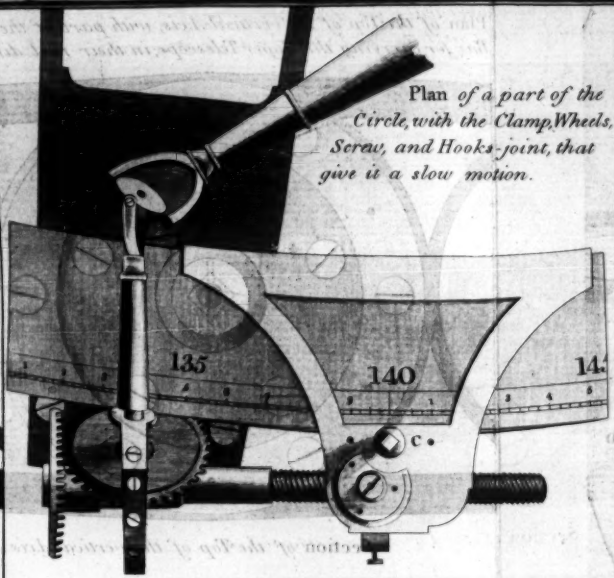
Scale *of Inches for parts represented to ½ of their real dimensions.*

Various parts of the Instrument represented to

ith part of the horizontal
n their real dimensions.

the vertical Axis.

Plan *of a part of the Circle, with the Clamp, Wheels, Screw, and Hooks-joint, that give it a slow motion.*



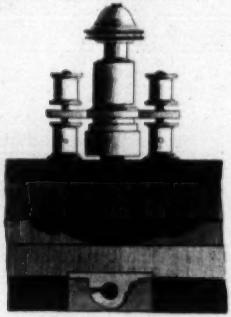
Milled-head-Key *placed at* C *for clamping the Instrument.*

Section *through one of t whereby the Instrument the three Mahogany Plan*

Scale *of Inches for parts represented in their real dimensions.*

Plan *of one of the Feet of the Instrument.*
Scale 1/3 of the real dimensions.

Front Elevation *of one of the Feet-Screws; shewing likewise one of the Blocks of box-wood, and one of the conical Rollers behind it.*



Front Section *of the Spring, and Conical Roller.*

e of the Feet-Screws,
nent is levelled; and of
Planes that are under it.

Under side of one of the Four Screws attached to the octagonal mahogany Plane for bringing the Center of the Instrument by the help of its plummet, over any point on the ground.

Scale *of Inches for parts represented to 1/3 of their real dimensions.*

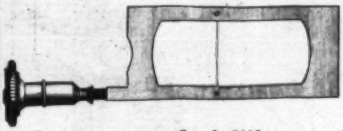
Pl. VI.

For the Explanation of the Microsc

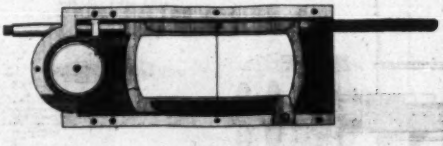
MICROSCOPE A.

Plans.

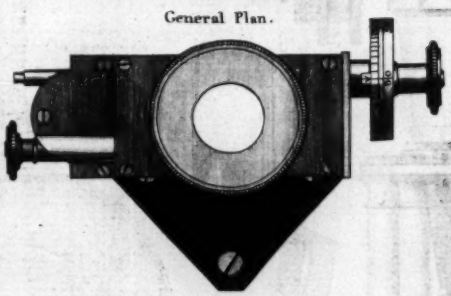
Upper or Brass Slide.



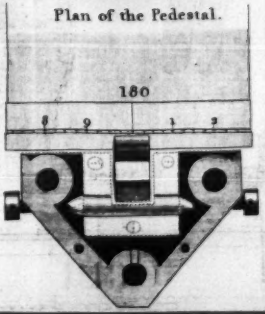
Steel Slide.



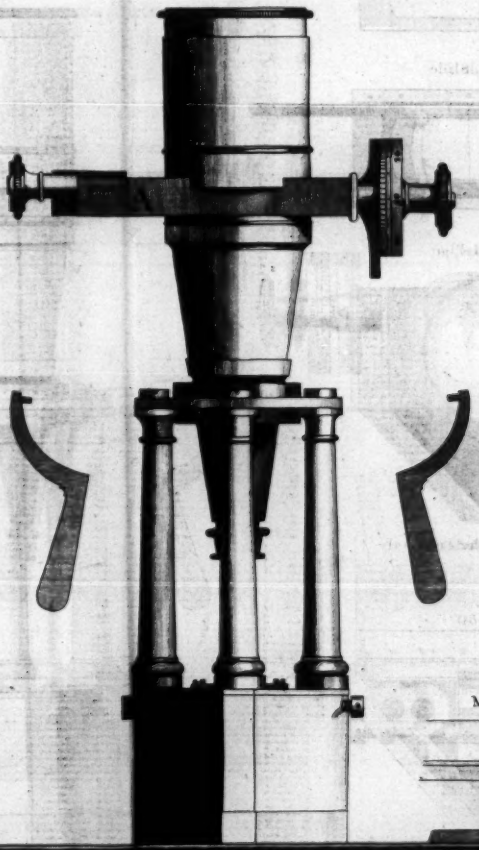
General Plan.



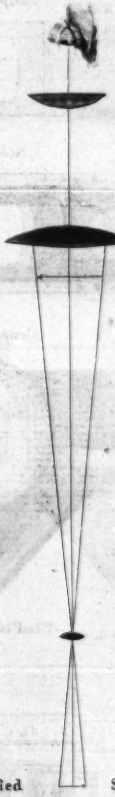
Plan of the Pedestal.



Elevation.

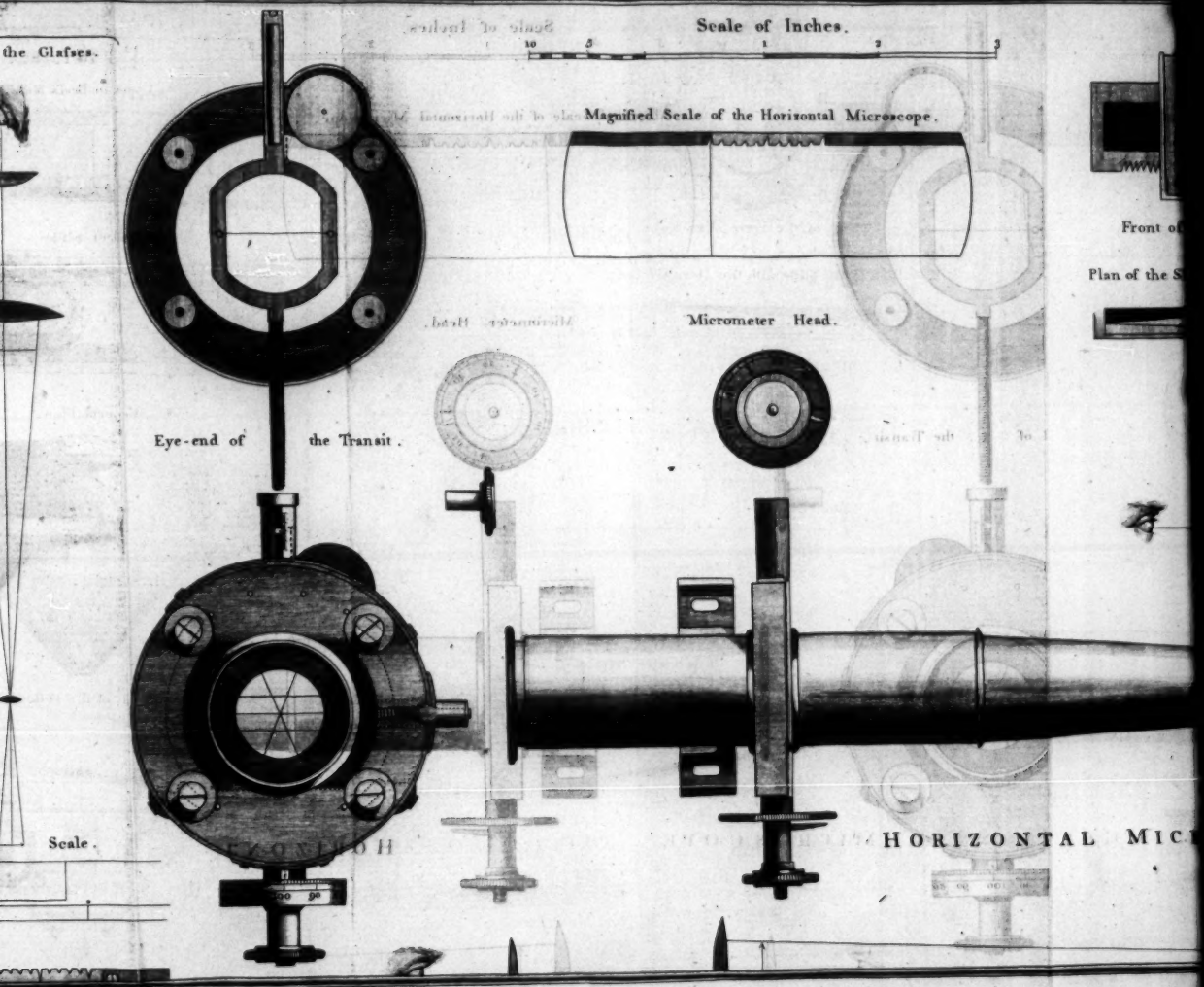


Position of the



Magnified

oscopes, and Mechanism for the Wires in the Eye-ends of the Telescopes.

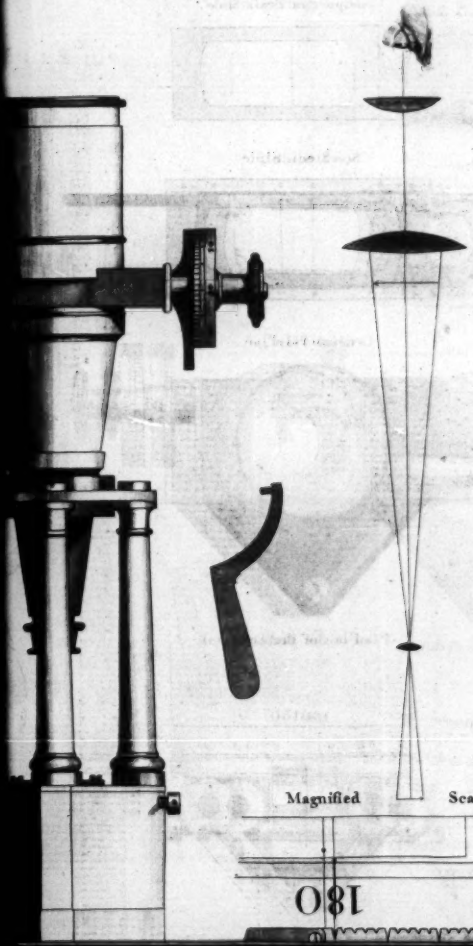


e Explanation of the Microscopes, and Mechanism for the Wires in the Eye

PE A.

evation.

Position of the Glaſses.



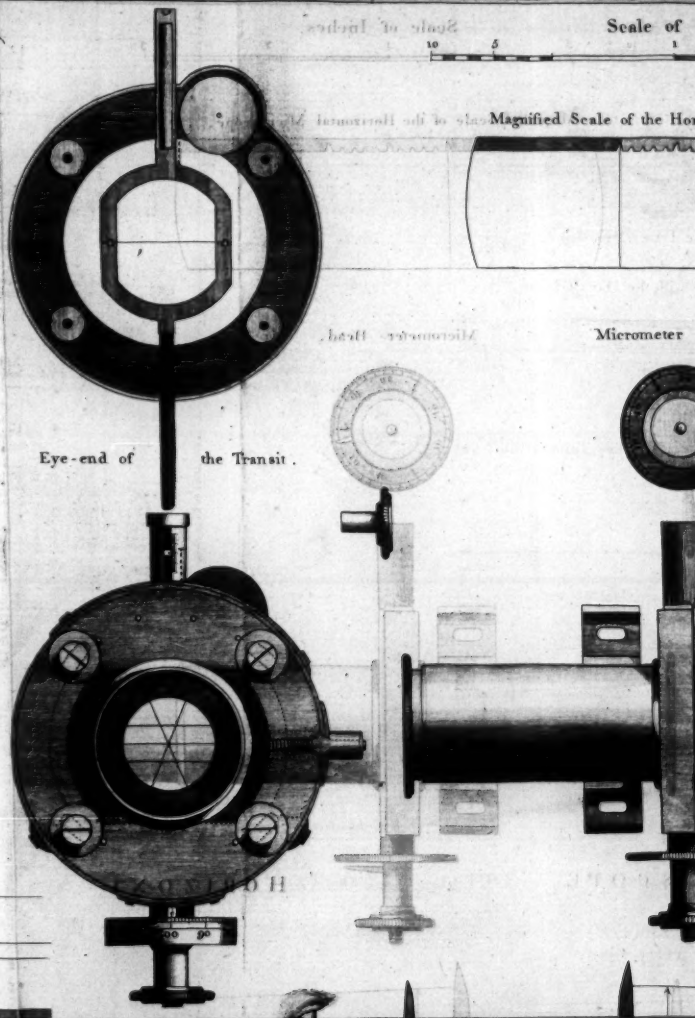
Magnified Scale.

Scale of

Magnified Scale of the Hor

Micrometer

Eye-end of the Transit.



Eye-ends of the Telescopes.

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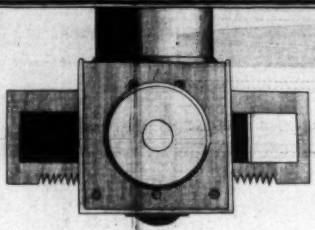
le of Inches.

1 2 3

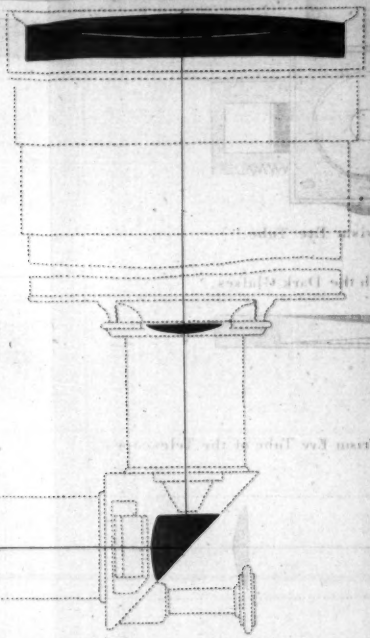
the Horizontal Microscope.

meter Head.

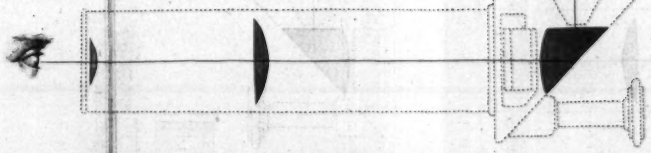


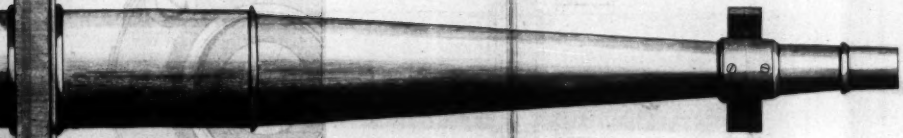
Front of the Prism Eye Tube.

Plan of the Slide with the Dark Glasses.



Prism Eye Tube of the Telescope.





HORIZONTAL MICROSCOPE.

Basire. Sc.

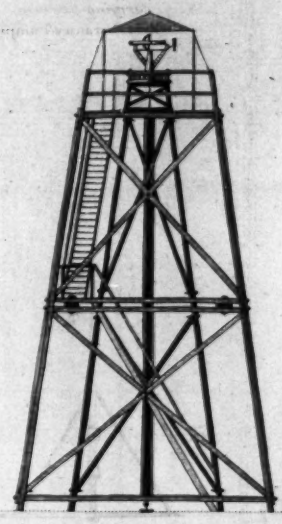
VARIOUS ARTICLES OF MACHINERY USE

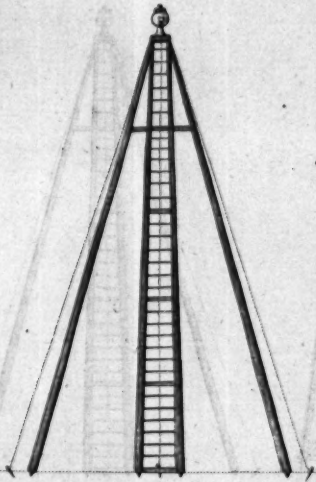
Portable Scaffold

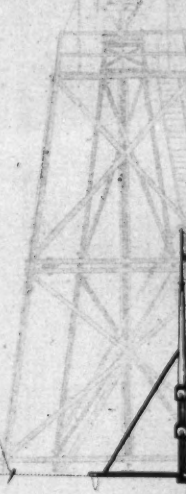
Tripod Ladder

Flag
carrying
Reverbera

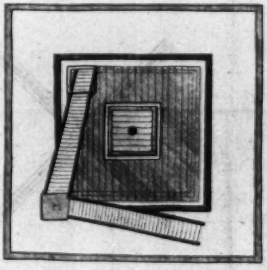
Elevations

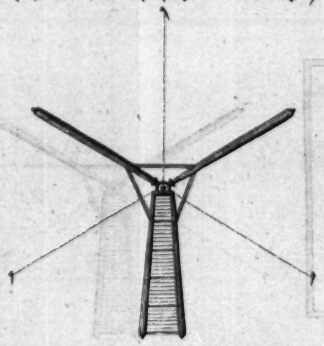


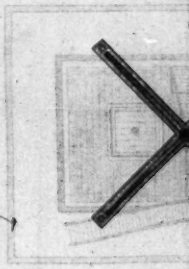




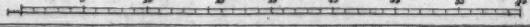
Plans







Scale of Feet.



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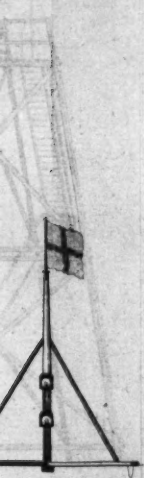
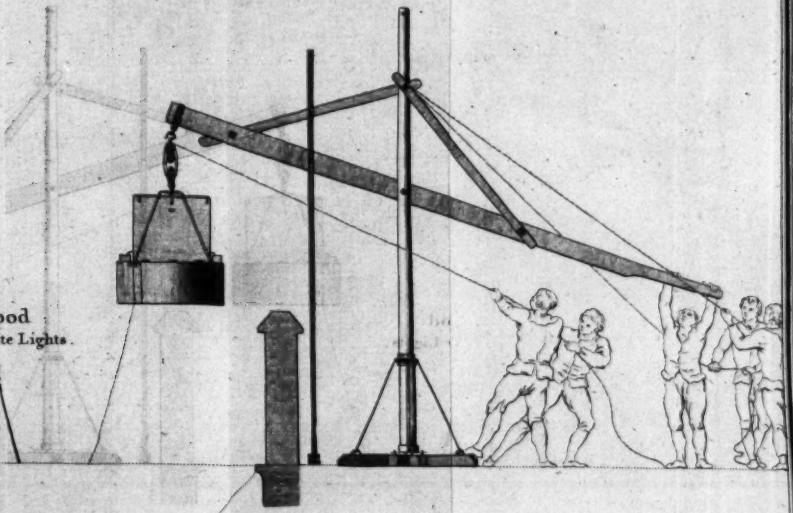
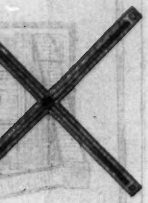
Flagstaff.
rrying likewise
verberatory Lamps.

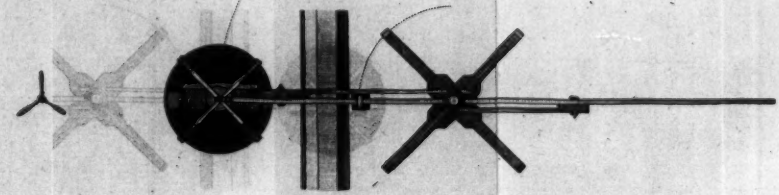
Portable Crane.





Tripod
for White Lights.



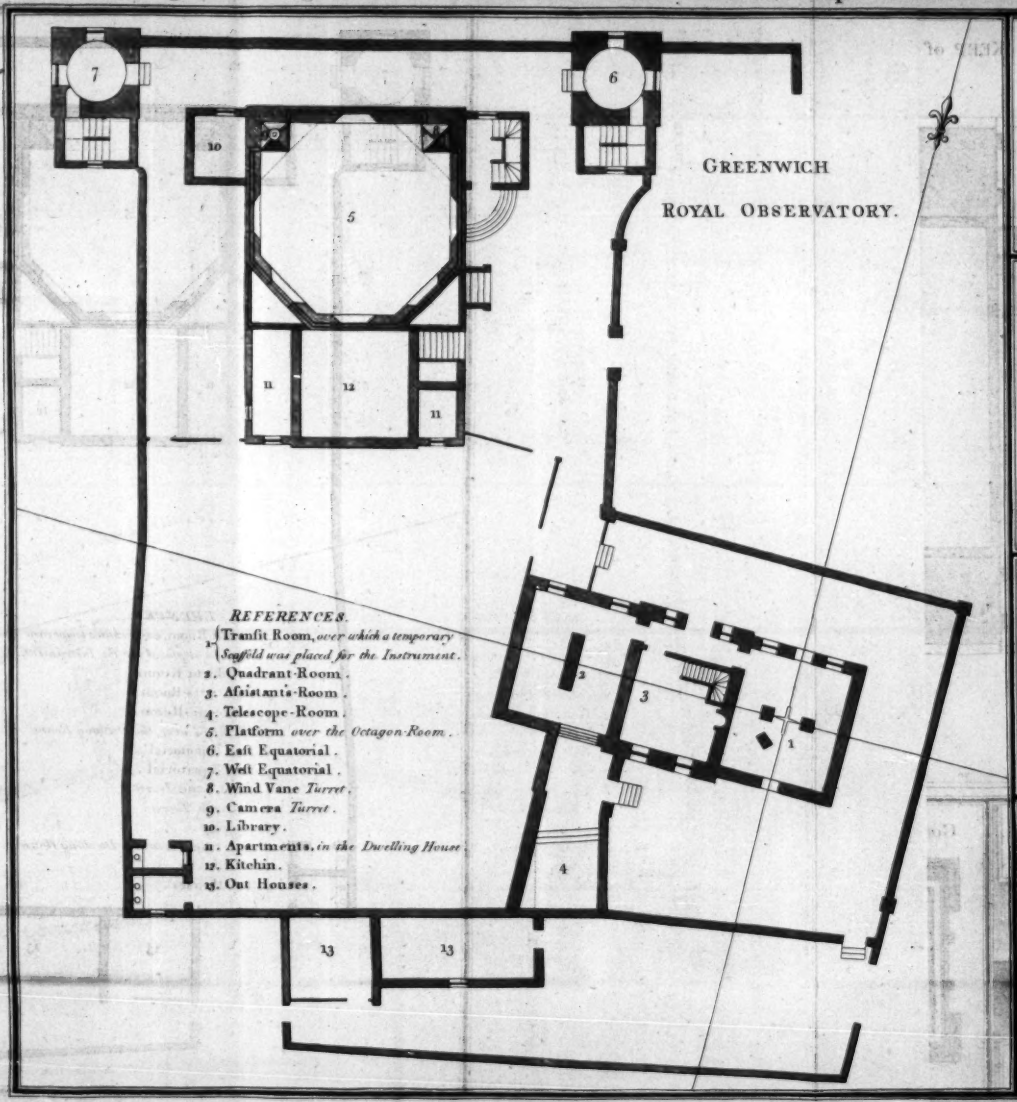


Scale of Feet for the Crane.

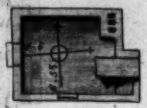
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20

Basire Sc.

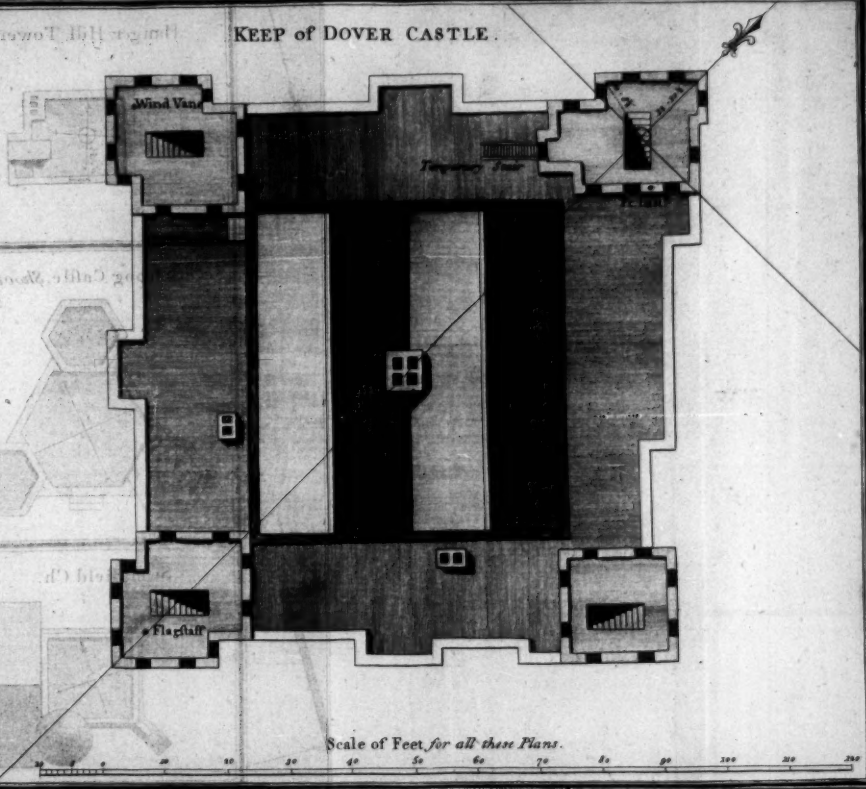
PLANS, shewing the Position of the INSTRUMENT on such Towers, Steeples of Churches, or



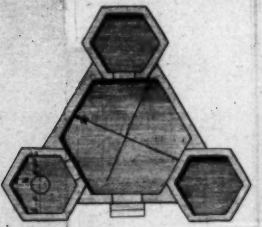
Hanger Hill Tower.



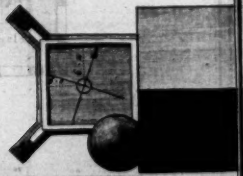
KEEP of DOVER CASTLE.



verndroog Caſtle, *Shooter's Hill*

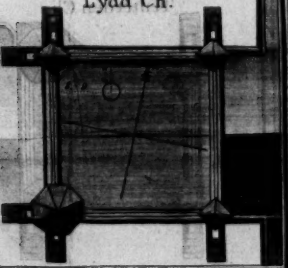


Swingfield Ch.

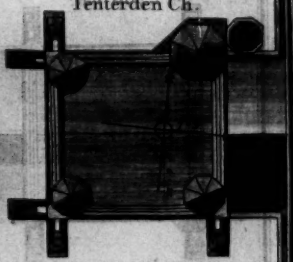


Scale of Feet *for all these Plans.*

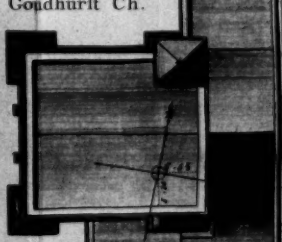
Lydd Ch.



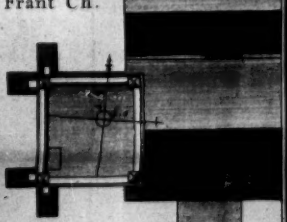
Tenterden Ch.



Goudhurſt Ch.



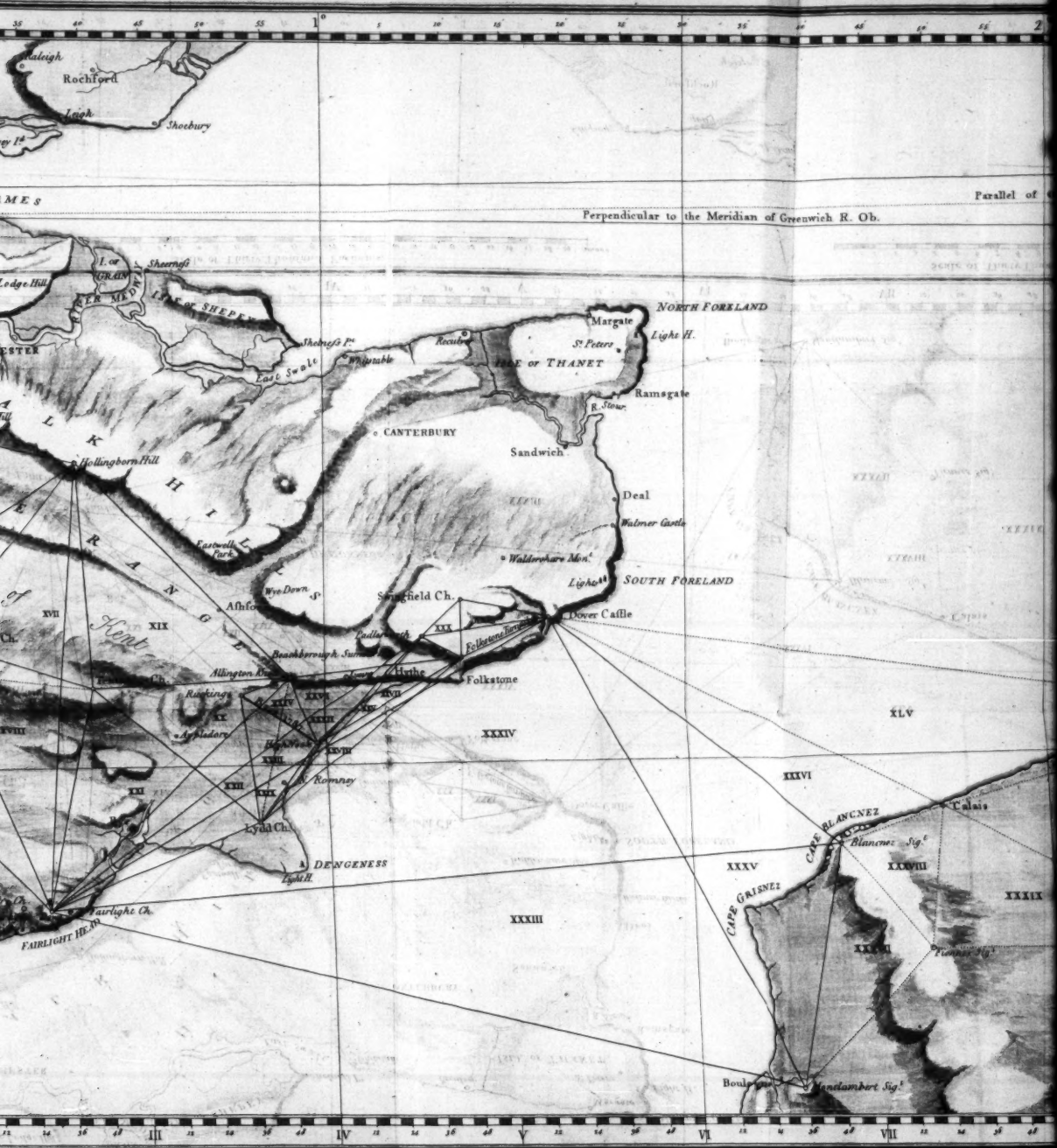
Frant Ch.



Pl. IX.

PLAN of the TRIANGLES whereby the DISTANCE

CE between the ROYAL OBSERVATORIES of GREENWICH and PARIS has bee



NGLES whereby the DISTANCE between the ROYAL OBSERVATORIES of G

of GREENWICH and PARIS has been determined

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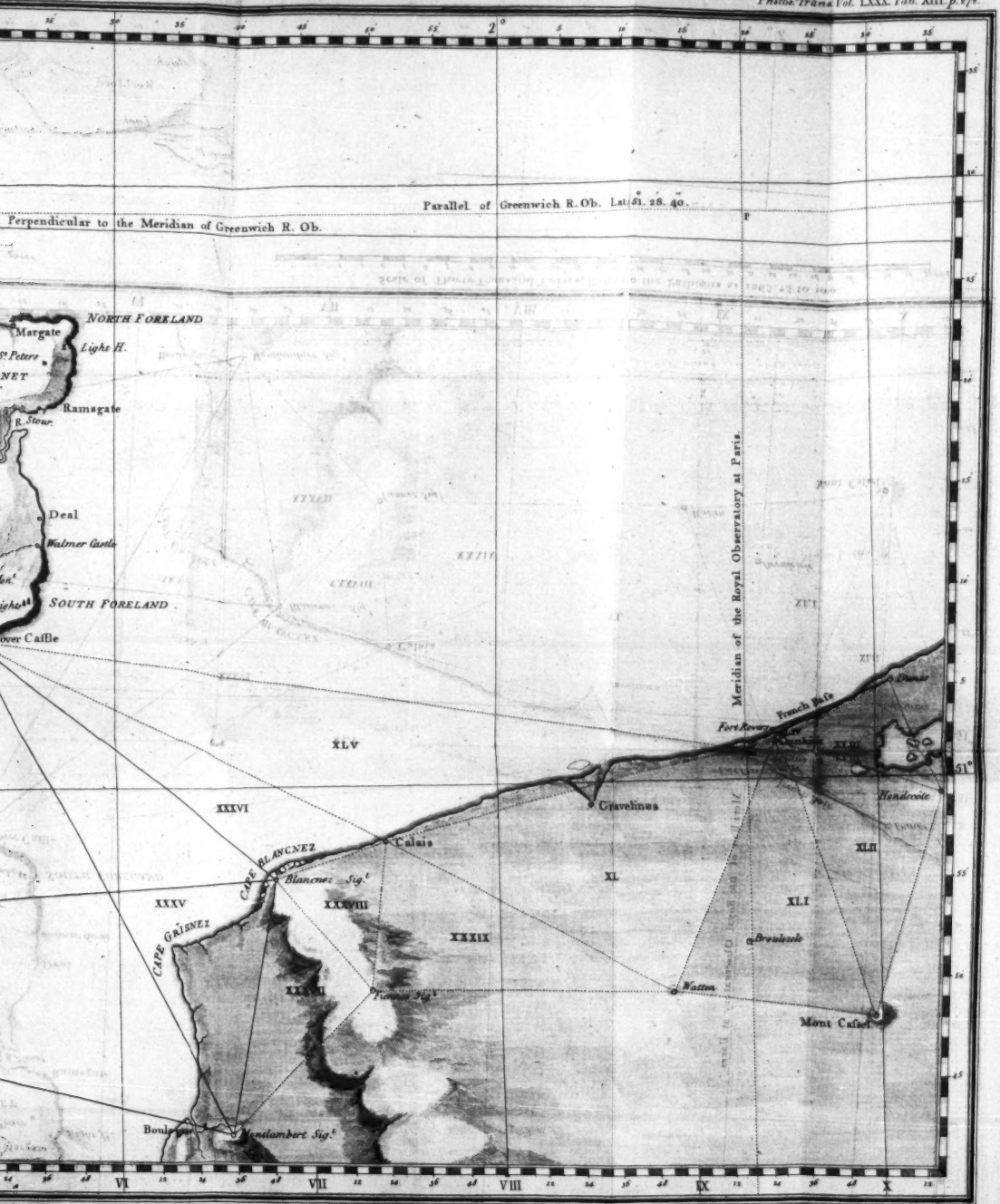


Fig. 1.

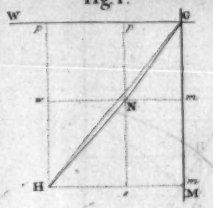
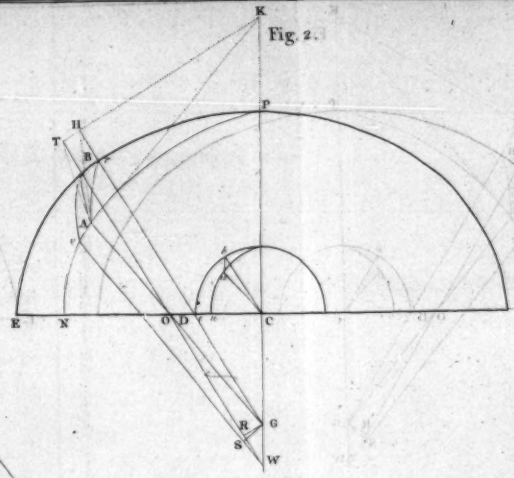


Fig. 2.



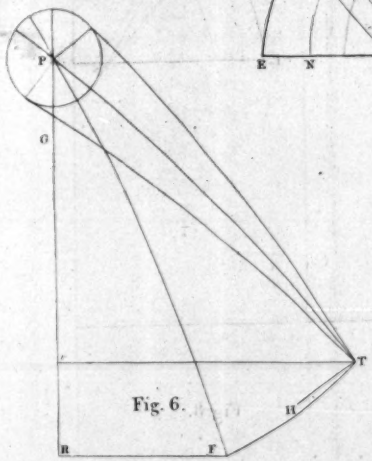


Fig. 6.

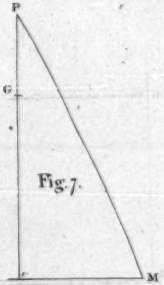


Fig. 7.

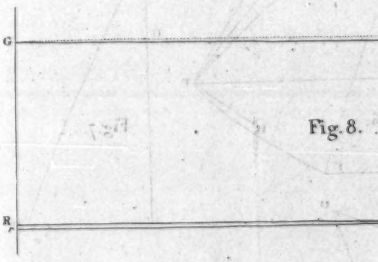


Fig. 8.

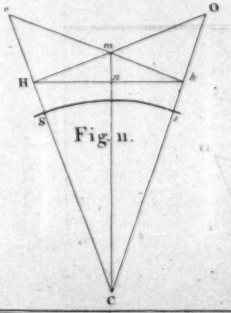


Fig. 11.

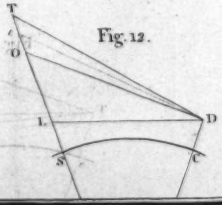


Fig. 12.

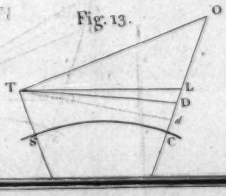


Fig. 13.

Fig 3.

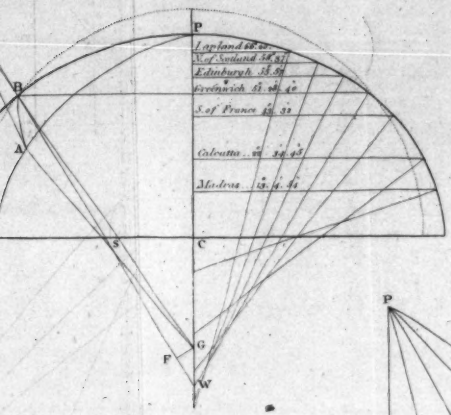


Fig. 4.

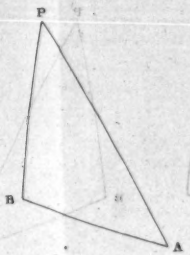
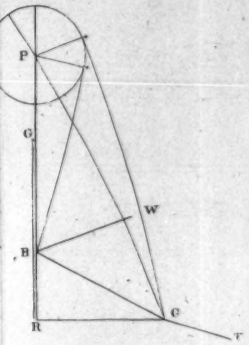
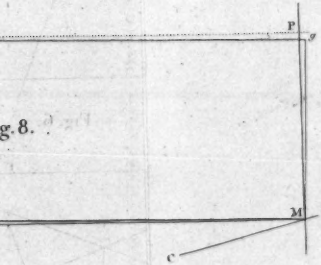


Fig. 5.





g. 8.

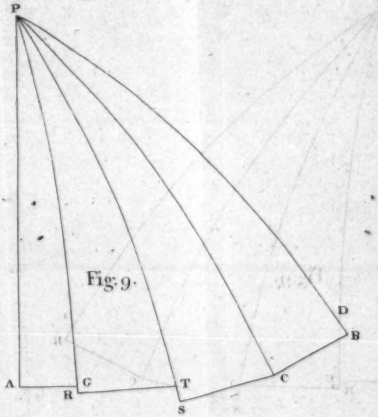


Fig. 9.

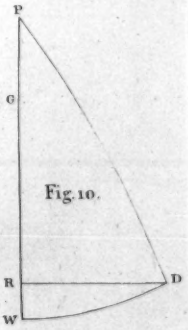


Fig. 10.

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Fig. 14.

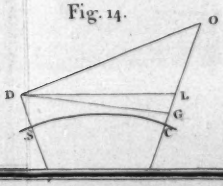


Fig. 15.

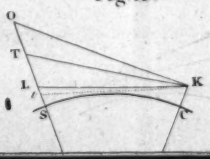
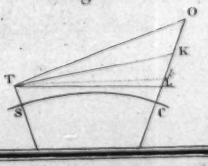


Fig. 16.



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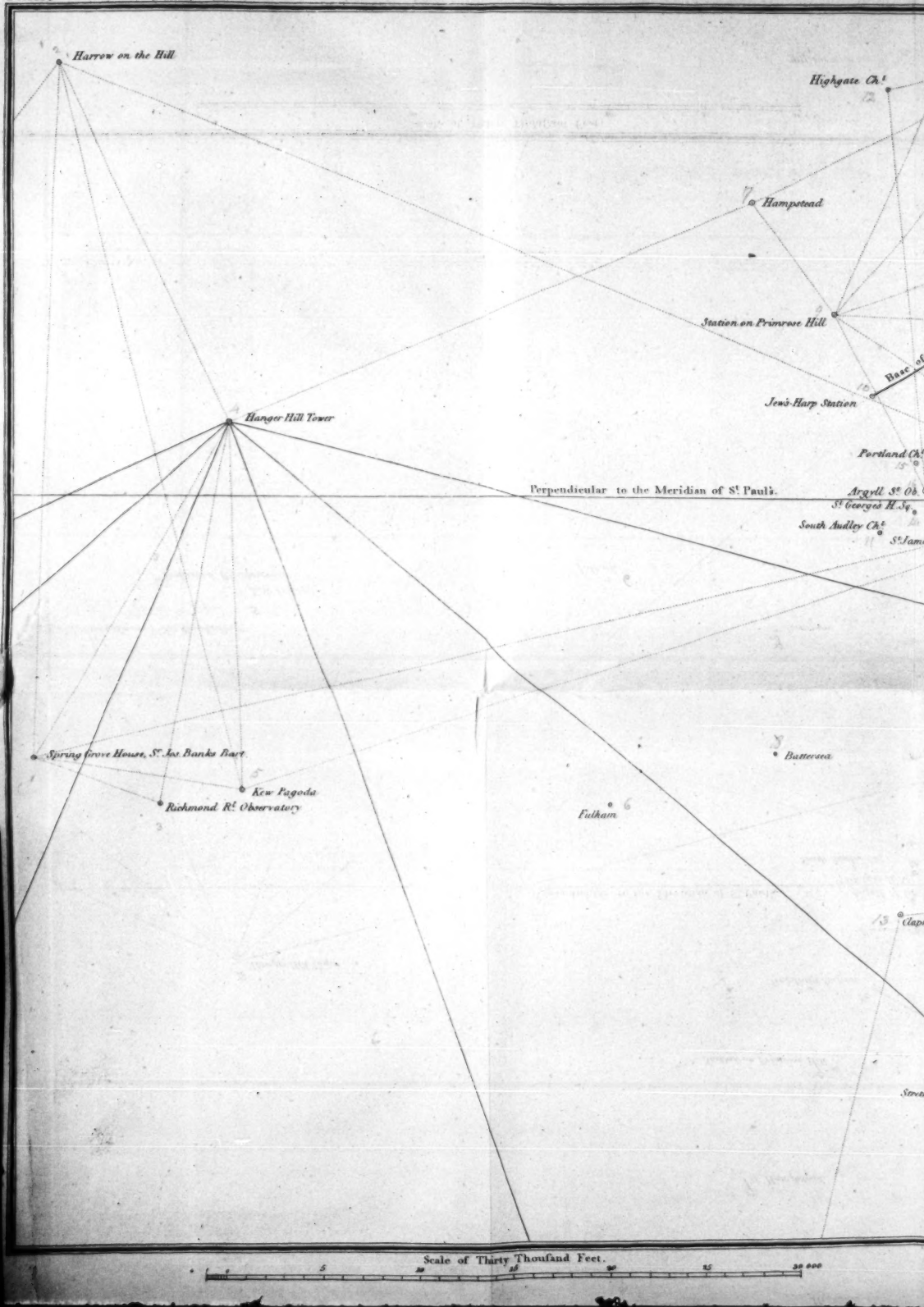

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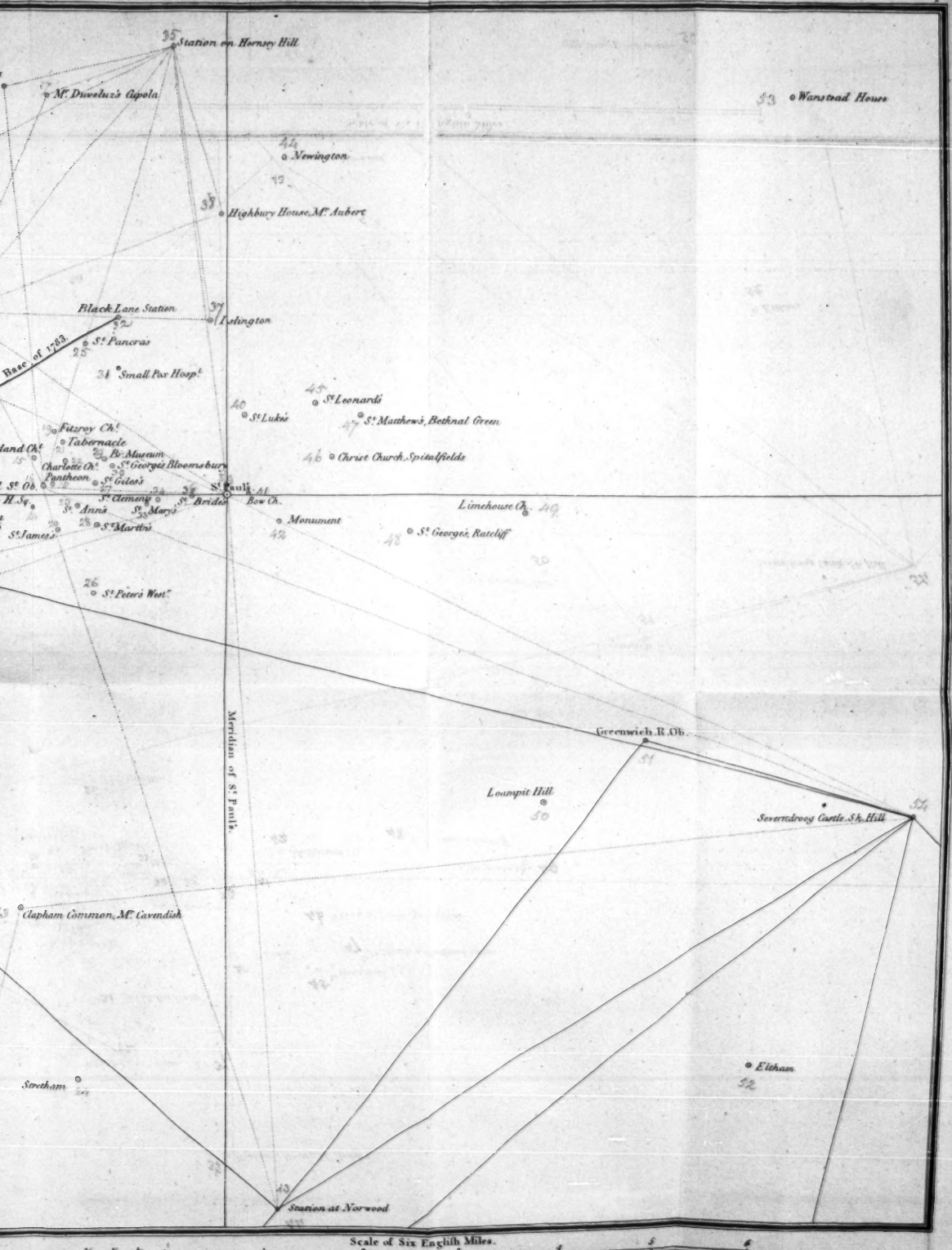
Philos. Trans. Vol. LXXX. Tab. XV. p. 271.



METEOROLOGICAL JOURNAL

KEPT AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January 1789.

1789	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Jan. 1	8	0	30	41	29,50		SW by W	1	Cloudy.
	2	0	35	43	29,48		WNW	1	Cloudy.
2	8	0	25	41	29,93		WNW	1	Cloudy.
	2	0	30	44	30,00		WNW	1	Cloudy.
3	8	0	22	40	30,30				Foggy.
	2	0	30	44	30,37		SE	1	Cloudy.
4	8	0	24	40	30,57		ESE	1	Cloudy.
	2	0	26	43	30,61		ESE	1	Cloudy.
5	8	0	17,5	39	30,73		NE	1	Fine.
	2	0	24,5	42	30,75		ENE	1	Fine.
6	8	0	29	39	30,54		ESE	2	Cloudy.
	2	0	27	41	30,58		ESE	2	Snow.
7	8	0	21	36	30,35		ENE	2	Fine.
	2	0	26	39	30,21		ENE	2	Fine.
8	8	0	21	36	30,00		NE	1	Fine.
	2	0	27	40	29,88		NE by E	1	Fine.
9	8	0	24	37	29,54		ENE	1	Snow.
	2	0	26	40	29,48				Snow.
10	8	0	24	38	29,34				Snow.
	2	0	28	40	29,28		W	1	Snow.
11	8	0	27	39	29,44				Cloudy.
	2	0	29	41	29,55		N by E	1	Cloudy.
12	8	0	21	38	29,74		N	1	Fine.
	2	0	25	41	29,79		NNW	1	Fine.
13	8	0	23	37	29,30		E	2	Snow.
	2	0	30	41	29,10		E by S	1	Cloudy.
14	8	0	35	40	29,07	0,525	SSW	2	Fair.
	2	0	37	46	29,17		SSW	2	Fine.
15	8	0	37	43	28,97		S by W	2	Cloudy.
	2	0	40	46	29,23		SW	2	Fair.
16	8	0	34	47	29,66		W	2	Fine.
	2	0	40	47	29,79		WSW	2	Fair.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Jan. 17	8	0	38	46	29,60	0,030	SE	2	Cloudy.
	2	0	39	47	29,44		SSE	2	Cloudy.
18	8	0	41	46	28,75		SSW	2	Cloudy.
	2	0	42	49	28,58		S	2	Cloudy.
19	8	0	36	48	29,20		W	1	Fair.
	2	0	42	50	29,33		W	2	Fine.
20	8	0	37	48	29,41	0,270	W	1	Cloudy.
	2	0	39	51	29,56		W	1	Fine.
21	8	0	40	48	29,71		WSW	1	Fine.
	2	0	44	51	29,78		SW	1	Rain.
22	8	0	45	50	29,61	0,120	SW	2	Rain.
	2	0	47	52	29,65		SW	2	Rain.
23	8	0	42	51	29,44	0,057	S	1	Cloudy.
	2	0	46	53	29,34		S by E	1	Cloudy.
24	8	0	43	49	29,34		S	2	Cloudy.
	2	0	43	53	29,19		S	2	Rain.
25	8	0	41	52	29,28	0,158	WSW	1	Fair.
	2	0	46	53	29,41		WSW	1	Fair.
26	8	0	42	53	29,66	0,123	SW	1	Cloudy.
	2	0	53	53	29,69		SW	1	Cloudy.
27	8	0	47	54	29,94		WSW	1	Cloudy.
	2	0	51	55	29,90		SW	1	Cloudy.
28	8	0	46	54	29,78	0,062	SW	2	Rain.
	2	0	52	54	29,83		SW	1	Cloudy.
29	8	0	43	53	30,23		SW by W	1	Foggy.
	2	0	46	54	30,31		SW by S	1	Cloudy.
30	8	0	42	54	30,33		S	1	Cloudy.
	2	0	47	55	30,30		S	1	Cloudy.
31	8	0	46	54	30,22		SW by W	1	Fair.
	2	0	54	56	30,14		SW by S	1	Fair.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Feb. 1	8	0	43	54	29.94		SSW	1	Cloudy.
	2	0	49	55	29,80		SSW	1	Fair.
2	8	0	43	54	29,63		SSW	1	Fair.
	2	0	48	56	29,68		SSW	1	Fair.
3	8	0	46	54	29,42	0,205	SSW	2	Cloudy.
	2	0	51	57	29,38		S by W	2	Cloudy.
4	8	0	39	54	29,68		WSW	1	Fair.
	2	0	40,5	54	29,38		S by W	2	Rain.
5	8	0	36,5	51,5	29,61	0,038	W	2	Fine.
	2	0	44	54	29,62		WNW	2	Fair.
6	8	0	36	52	29,80		W by N	1	Fine.
	2	0	45	54	29,83		W by N	1	Fine.
7	8	0	42	52	29,46	0,200	W by N	1	Fair.
	2	0	43	53	29,63		WNW	1	Cloudy.
8	8	0	35	51	29,78		W	1	Fair.
	2	0	43,5	52	29,63		SW by S	1	Rain.
9	8	0	36,5	52	29,65	0,041	W by N	1	Fine.
	2	0	44	52	29,62		W by N	1	Fair.
10	8	0	35	50	29,63		WSW	1	Fine,
	2	0	42,5	54	29,43		SW by W	1	Fair.
11	8	0	35,5	51	29,51	0,040	SW	1	Fair.
	2	0	40,5	52,5	29,55		NWbyW	1	Cloudy.
12	8	0	34	49	30,00		NWbyW	1	Fair.
	2	0	40,5	52	30,10		WNW	1	Fair.
13	8	0	37,5	50	30,19		W by S	1	Cloudy.
	2	0	44,5	52	30,19		W by S	1	Cloudy.
14	8	0	41	50	30,18		W by S	1	Cloudy.
	2	0	48	52	30,11		WSW	2	Cloudy.
15	8	0	46	51	30,00		SW by W	2	Cloudy.
	2	0	49	52,5	29,95		WSW	2	Cloudy.
16	8	0	38	51,5	29,99	0,155	W by N	1	Fine.
	2	0	43,5	52,5	30,08		WNW	2	Fair.

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for February 1789.

1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Feb. 17	8	0	36	50,5	30,34		SW	1	Cloudy.
	2	0	44	52	30,10		WSW	2	Cloudy.
18	8	0	44	51	30,11		W by S	2	Fair.
	2	0	50,5	53	30,11		W	2	Cloudy.
19	8	0	46	51	30,00		SW	1	Cloudy.
	2	0	51	53	29,91		SW by S	2	Cloudy.
20	8	0	39	53	30,09		E	1	Fair.
	2	0	44	55	30,11		E	1	Fair.
21	8	0	42	52,5	29,66	0,141	SSW	2	Rain
	2	0	48,5	55	29,59		SW	2	Fair.
22	8	0	46	53	29,16	0,100	SW by S	2	Cloudy.
	2	0	47,5	55,5	29,33		SW	2	Fair.
23	8	0	42	52,5	29,72	0,155	SW	2	Cloudy.
	2	0	45,5	54	29,51		S	2	Cloudy.
24	8	0	38,5	49	29,39	0,153	SSW	2	Fine.
	2	0	48	52,5	29,39		SW	2	Cloudy.
25	8	0	41,5	51	29,18	0,055	S by W	2	Cloudy.
	2	0	45,5	54	28,65		SW	2	Cloudy.
26	8	0	37,5	51	28,69	0,195	WSW	1	Rain.
	2	0	41	53,5	28,90		WSW	2	Cloudy.
27	8	0	39	50,5	29,55	0,127	WNW	2	Cloudy.
	2	0	43,5	53	29,58		N	1	Cloudy.
28	8	0	38	51	29,73		NNE	2	Cloudy.
	2	0	42	52	29,84		N by E	2	Cloudy.

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1789	Time. H.	M.	Therm without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 1	7	0	35,5	49	30,08		NNE	1	Cloudy.
	2	0	41	52	30,11		NNE	1	Cloudy.
2	7	0	35,5	49	30,11		NNE	1	Cloudy.
	2	0	38	50,5	30,08		N	1	Cloudy.
3	7	0	31,5	48,5	30,13		NNE	1	Fair.
	2	0	43	51	30,08		NNE	1	Cloudy.
4	7	0	33	48	29,91		N	1	Fair.
	2	0	38	50,5	29,87		NE	1	Cloudy.
5	7	0	33,5	48	29,85		NE	1	Cloudy.
	2	0	38	50	29,89		NE	1	Cloudy.
6	7	0	31,5	47	30,08		NE	1	Cloudy.
	2	0	37	48	30,10		NE	1	Cloudy.
7	7	0	26	45	30,00		NE	1	Fair.
	2	0	31,5	47	29,98		NE	1	Cloudy.
8	7	0	29	44,5	30,02		N	1	Snow.
	2	0	31,5	49	30,02		N	1	Cloudy.
9	7	0	30,5	45	29,95		WNW	1	Cloudy.
	2	0	41	49	29,82		WNW	1	Fine.
10	7	0	26	44	29,48		NW by W	2	Fine.
	2	0	38,5	48,5	29,56		NW by N	2	Fine.
11	7	0	30,5	45,5	29,35		E	1	Cloudy.
	2	0	32	47,5	29,25		E	2	Cloudy.
12	7	0	26	44,5	29,31		N	1	Fair.
	2	0	34	47	29,36		NW	1	Fair.
13	7	0	28,5	44,5	28,96		E	1	Snow.
	2	0	40	49	28,94		W	1	Fair.
14	7	0	36	46	29,06	0,285	SW by S	1	Cloudy.
	2	0	42	50	29,14		SW by S	1	Rain.
15	7	0	37	46,5	29,21	0,025	E	1	Cloudy.
	2	0	40	49,5	29,26		E	1	Rain.
16	7	0	34	46	29,62	0,275	E	2	Rain.
	2	0	35	48	29,73		NE by N	2	Cloudy.

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1789	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	°	°	Inches.	Inch.	Points.	Str.	
Mar. 17	7 0	32,5	46	29,82		NE	1	Cloudy.
	2 0	37,5	48	29,82		SW	1	Cloudy.
18	7 0	33,5	44,5	29,38	0,036	SSE	2	Rain.
	2 0	35,5	47	29,30		ESE	1	Rain.
19	7 0	34,5	45	29,64	0,043	E	1	Fair.
	2 0	40	47	29,74		E	1	Cloudy.
20	7 0	35	46	29,66		SSW	1	Cloudy.
	2 0	42	49	29,50		SW	1	Rain.
21	7 0	35	47	29,54	0,394	W by S	1	Fine.
	2 0	46,5	50	29,61		W	1	Fair.
22	7 0	38	47,5	29,30	0,180	E	1	Cloudy.
	2 0	46,5	50,5	29,22		SE by S	1	Rain.
23	7 0	36	48	29,70	0,301	NW	1	Cloudy.
	2 0	38	50	29,93		N	1	Cloudy.
24	7 0	29	47,5	30,03		E	1	Cloudy.
	2 0	38,5	51	29,96		SE	1	Fair.
25	7 0	33	47	29,50		E	2	Cloudy.
	2 0	34	49	29,41		E	1	Snow.
26	7 0	32,5	46,5	29,73	0,010	NE	1	Cloudy.
	2 0	38,5	49	29,87		NE	1	Cloudy.
27	7 0	29,5	46,5	29,99		WNW	1	Hazy.
	2 0	38	49	29,96		SW	1	Fair.
28	7 0	34,5	47	29,78		SE by S	1	Hazy.
	2 0	37,5	48,5	29,69		S by E	1	Cloudy.
29	7 0	35	47	29,57		E	1	Foggy.
	2 0	38,5	49	29,62		E	1	Cloudy.
30	7 0	33,5	46,5	29,93		NNW	1	Fair.
	2 0	41	49	30,00		NW	1	Cloudy.
31	7 0	36	46,5	30,00		W	1	Fine.
	2 0	45,5	49,5	29,96		WNW	1	Cloudy.

METEOROLOGICAL JOURNAL
for April 1789.

1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
April 1	7	0	43,5	48,5	29 55	0,223	W	1	Fair.
	2	0	50	51,5	29,65		W	1	Hazy.
2	7	0	39	48	29,39	0,107	SE	1	Rain.
	2	0	53	52	29,18		SW	2	Cloudy.
3	7	0	44	51	29,10	0,120	SW	1	Rain.
	2	0	50	53	29,21		WSW	1	Cloudy.
4	7	0	35,5	50,5	29,59	0,028	N by E	1	Cloudy.
	2	0	40	52,5	29,78		NNW	1	Cloudy.
5	7	0	33,5	49	30,11		SW	1	Fine.
	2	0	44	51,5	30,13			0	Fine.
6	7	0	39,5	49,5	30,13		E	1	Cloudy.
	2	0	46	54,5	30,13		E	1	Fair.
7	7	0	34,5	49,5	30,05		SE	1	Fair.
	2	0	48,5	53	29,97		W	1	Cloudy.
8	7	0	40,5	51	30,00		W	1	Cloudy.
	2	0	49,5	53	30,03		WNW	1	Cloudy.
9	7	0	35,5	51	30,17		E	1	Foggy.
	2	0	48	54,5	30,17		E	1	Fine.
10	7	0	37	52,5	30,09		E	1	Cloudy.
	2	0	44	54	30,00		E	1	Fair.
11	7	0	38	50,5	29,82		WSW	1	Hazy.
	2	0	52	53	29,77		WNW	1	Cloudy.
12	7	0	42	52,5	29,79		NNE	1	Fine.
	2	0	50	56	29,78		N	1	Fine.
13	7	0	36	53	29,96		NE	1	Fine.
	2	0	50	55	29.97		ENE	1	Fine.
14	7	0	36	52	30,06		E	1	Fine.
	2	0	47	55	30,00		E	1	Fine.
15	7	0	38	53	29,83		E	1	Fine.
	2	0	55	56,5	29,73		E	1	Fine.
16	7	0	43	54	29,70		SW	1	Fine.
	2	0	60	56	29,70		SSW	2	Fine.

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METEOROLOGICAL JOURNAL
for April 1789.

1789	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
April 17	7	0	48	56	29,49	0,144	SW by S	2	Rain.
	2	0	58	59	29,54		SW	2	Fair.
18	7	0	46	56	29,62	0,040	WSW	2	Fine.
	2	0	57	59	29,62		WSW	2	Fair.
19	7	0	45,5	55,5	29,78		WSW	1	Cloudy.
	2	0	56,5	58	29,85		WSW	1	Cloudy.
20	7	0	46	56	30,12		WSW	1	Fine.
	2	0	61	61	30,12		WSW	1	Fine.
21	7	0	52,5	57,5	30,18		WSW	1	Cloudy.
	2	0	61,5	60,5	30,15		WSW	1	Cloudy.
22	7	0	50	57	30,05		WSW	1	Fine.
	2	0	62	60	30,11		WSW	1	Fair.
23	7	0	46,5	56	29,82	0,045	SW	1	Rain.
	2	0	56	59	29,73		SW	1	Cloudy.
24	7	0	46	56,5	29,48	0,025	WNW	2	Cloudy.
	2	0	56	60	29,74		WNW	1	Fair.
25	7	0	44	56	29,84		SW by W	1	Fair.
	2	0	57	59	29,70		SW	1	Cloudy.
26	7	0	42,5	56	29,33	0,085	SSW	2	Fine.
	2	0	51,5	58	29,27		SW	2	Cloudy.
27	7	0	44	55,5	29,24	0,091	SW	2	Fine.
	2	0	50	56	29,26		SW	2	Cloudy.
28	7	0	46	54	29,50	0,049	SW by W	2	Fair.
	2	0	55	57	29,59		WSW	2	Fair.
29	7	0	45	55	29,68		WSW	1	Fair.
	2	0	55,5	58	29,67		SW by S	1	Fine.
30	7	0	43,5	54,5	29,74		E	1	Fine.
	2	0	57	58,5	29,72		E	1	Fine.

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1789	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
May 1	7	0	47,5	55,5	29,79		ENE	1	Cloudy.
	2	0	56	58	29,79		ENE	1	Cloudy.
2	7	0	46,5	55	29,93	0,170	E	1	Cloudy.
	2	0	51	58	29,93		E	1	Cloudy.
3	7	0	45	56	29,75	0,070	E	2	Rain.
	2	0	50	58	29,61		E	1	Cloudy.
4	7	0	50	56	29,61	0,045	E	1	Cloudy.
	2	0	59,5	60	29,68		SSW	1	Fair.
5	7	0	48	56	29,73		S by W	1	Fine.
	2	0	61	59	29,73		SSW	2	Fair.
6	7	0	53	58	29,74		SSW	1	Cloudy.
	2	0	61,5	61	29,84		SW by W	1	Fair.
7	7	0	52	58,5	30,22		S by W	1	Hazy.
	2	0	63	61,5	30,23		ESE	1	Fine.
8	7	0	53,5	59	30,25		E	1	Fine.
	2	0	64,5	62	30,18		E	1	Fine.
9	7	0	55	61	30,03		S	1	Fine.
	2	0	66	63,5	29,99		S	1	Fine.
10	7	0	55	62	30,18		WSW	1	Fine.
	2	0	65	63,5	30,18		W	1	Fine.
11	7	0	55,5	62,5	30,25		NE	1	Fine.
	2	0	65	64	30,23		E	1	Fine.
12	7	0	54	63	30,21		E	1	Fine.
	2	0	60	63	30,17		E	2	Fine.
13	7	0	54	62	29,98		E by N	1	Fine.
	2	0	66	64	29,86		E	1	Fine.
14	7	0	57	62	29,92		WSW	2	Fine.
	2	0	64	65,5	29,91		WSW	1	Fine.
15	7	0	56	62,5	29,68		E	1	Rain.
	2	0	62	63	29,65		SW	1	Cloudy.
16	7	0	55	62	29,65		WSW	1	Cloudy.
	2	0	61	63	29,86		W	1	Cloudy.

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1789	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
May 17	7	0	55	61	29,92	0,115	SSW	2	Cloudy.
	2	0	53,5	61,5	29,84		SSW	2	Cloudy.
18	7	0	52	60	29,72	0,055	SSW	2	Cloudy.
	2	0	55	60	29,85		WSW	2	Cloudy.
19	7	0	47	58	30,22		W	2	Fine.
	2	0	59	59,5	30,27		W	2	Fair.
20	7	0	50,5	59	30,16		E	1	Fine.
	2	0	61	60	30,04		SE	1	Fine.
21	7	0	52	58,5	29,90		E	1	Cloudy.
	2	0	55	58	29,90		E	1	Rain.
22	7	0	54	58,5	29,89		SSE	1	Fair.
	2	0	58	59	29,87		S by W	1	Cloudy.
23	7	0	55	58,5	29,91	0,165	SSW	1	Fair.
	2	0	63	60,5	29,96		SSW	1	Fine.
24	7	0	56	59,5	29,78	0,137	E	1	Cloudy.
	2	0	66	61,5	29,69		E	1	Cloudy.
25	7	0	60	60,5	29,57		E	1	Cloudy.
	2	0	65	61,5	29,59		ESE	1	Cloudy.
26	7	0	58	61	29,67		ESE	1	Fine.
	2	0	64	62	29,64		ESE	1	Fair.
27	7	0	55	61	29,67		S	1	Cloudy.
	2	0	62	62	29,70		S	1	Fair.
28	7	0	54	61	29,89		SSW	1	Cloudy.
	2	0	62	62	29,89		SW by S	1	Fair.
29	7	0	55	61,5	29,74	0,025	SW	2	Cloudy.
	2	0	61	62	29,74		SSW	1	Fair.
30	7	0	55	61	29,73		S	2	Fair.
	2	0	60	61,5	29,67		S	1	Cloudy.
31	7	0	54	60	29,57	0,321	SW	1	Fair.
	2	0	60	61,5	29,60		SSW	2	Cloudy.

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1789	Time. H.	Time. M.	Therm. without °	Therm within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
June 1	7	0	54	60	29,70	0,223	SW	2	Fair.
	2	0	59	60,5	29,70		SW	2	Cloudy.
2	7	0	52	60	29,86		WSW	1	Cloudy.
	2	0	66	61,5	29,86				Fine.
3	7	0	55	60,5	29,89		WNW	1	Fine.
	2	0	63	61,5	29,78		WSW	1	Cloudy.
4	7	0	52	60	29,40	0,366	WSW	2	Fair.
	2	0	54	60	29,40		WSW	2	Rain.
5	7	0	53	60	29,51	0,280	WNW	1	Cloudy.
	2	0	54	59,5	29,72		W	1	Cloudy.
6	7	0	53	58,5	29,77	0,062	W	1	Cloudy.
	2	0	58	59,5	29,74		W	1	Cloudy.
7	7	0	53	59	29,93		WNW	1	Cloudy.
	2	0	60	59,5	29,99		NW	2	Cloudy.
8	7	0	50	58,5	30,16		NNW	1	Fair.
	2	0	62	59,5	30,16		NW	1	Fair.
9	7	0	57	59,5	30,11		WNW	1	Cloudy.
	2	0	64	59,5	30,04		WNW	1	Fair.
10	7	0	54	58,5	29,96	0,060	WNW	1	Fair.
	2	0	64	59,5	29,99		NW	1	Fair.
11	7	0	56	59	30,09		E	1	Cloudy.
	2	0	66	60,5	30,12		ENE	1	Cloudy.
12	7	0	54	58,5	30,22		ENE	1	Cloudy.
	2	0	63,5	60	30,22		ENE	1	Cloudy.
13	7	0	53	58	30,23		ENE	1	Cloudy.
	2	0	61,5	60,5	30,23		ENE	1	Fair.
14	7	0	50	58	30,17		N	1	Cloudy.
	2	0	53	60	30,10		NE	1	Cloudy.
15	7	0	55	58	30,03		ENE	1	Cloudy.
	2	0	68	62	29,98		E	1	Fine.
16	7	0	62	60	29,87		E	1	Fine.
	2	0	71,5	63	29,81		E	2	Fine.

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1789	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
June 17	7	0	62	63,5	29,79		E	2	Fine.
	2	0	72	64	29,81		S by E	1	Fair.
18	7	0	62	64	29.88		E	1	Cloudy.
	2	0	64	64	29,80		SE	1	Rain.
19	7	0	58	64	29,85	0,320	SW by S	1	Fine.
	2	0	70	65	29,80		S	1	Fine.
20	7	0	60	64	29,63	0,116	SSW	2	Cloudy.
	2	0	66	66	29,70		SW	2	Fair.
21	7	0	58,5	64,5	29,70		SSW	1	Fair.
	2	0	68	66	29,55		SSE	1	Cloudy.
22	7	0	54	63	29,45	0,286	S	2	Rain
	2	0	60	63,5	29,40		S	2	Cloudy.
23	7	0	54	63	29,53	0,143	SW	2	Fair.
	2	0	62	63	29.53		SW	2	Rain.
24	7	0	55	62,5	29,55	0,246	SW	1	Cloudy.
	2	0	61	63	29,52		S	1	Cloudy.
25	7	0	54	61,5	29,60	0,690	E	1	Cloudy.
	2	0	60	62	29,64		SW	1	Cloudy.
26	7	0	53	61	29,77	0,242	W	1	Fair.
	2	0	60	62	29,83		W	1	Fine.
27	7	0	53	61	29,93		WSW	1	Fair.
	2	0	59	61	29,87		SW by S	1	Cloudy.
28	7	0	50	60	29,71	0,130	W	2	Cloudy.
	2	0	58	60	29,78		W by S	2	Cloudy.
29	7	0	52	59,5	29,91	0,070	W by N	1	Cloudy.
	2	0	57	60	29,91		WNW	1	Fine.
30	7	0	54,5	59,5	30,00	0,010	W	1	Cloudy.
	2	0	61,5	60	30,04		NNW	1	Fair.

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1789	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
July 1	7	0	52	59	30,09	0,020	W	1	Cloudy.
	2	0	59,5	59,5	30,07		W	1	Cloudy.
2	7	0	56	59,5	30,02	0,040	W	1	Cloudy.
	2	0	66	61,5	29,96		WSW	1	Cloudy.
3	7	0	60	60,5	29,93		SSW	1	Fine.
	2	0	70	64	29,88		S	1	Fine.
4	7	0	62	63	29,77		SW	1	Cloudy.
	2	0	65	64	29,84		S	1	Cloudy.
5	7	0	60	63,5	29,78	0,050	W	1	Rain.
	2	0	60,5	64	29,76		SW	1	Cloudy.
6	7	0	59	63	29,87	0,684	S	1	Fair.
	2	0	65	64,5	29,87		S	1	Fine.
7	7	0	57	63	29,95		S by W	2	Fine.
	2	0	64	64	29,95		S by W	2	Fair.
8	7	0	58,5	63	29,87	0,130	SW	1	Fair.
	2	0	65 5	64,5	29,93		SW	2	Fine.
9	7	0	56,5	63	30,07		WSW	2	Fair.
	2	0	66	64	30,07		SW by S	2	Fair.
10	7	0	60	63,5	29,97		SW	1	Fair.
	2	0	65	64,5	29,90		SW	1	Cloudy.
11	7	0	59	62	29,88		SSW	2	Fair.
	2	0	65	63,5	29,88		SW	2	Fair.
12	7	0	60	63	29,80		SSW	2	Fair.
	2	0	65	64	29,72		SW	2	Cloudy.
13	7	0	61	63	29,54	0,170	ESE	1	Cloudy.
	2	0	65	64	29,54		SW	1	Cloudy.
14	7	0	61	63	29,80	0,660	W	1	Fair.
	2	0	66	65	29,82		W	1	Fair.
15	7	0	59	64	29,84		W	2	Fair.
	2	0	63	64	29,79		SSW	2	Cloudy.
16	7	0	58	63,5	29,75	0,081	WSW	1	Cloudy.
	2	0	66	64	29,72		SW by S	1	Fair.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
July 17	7	0	56	63,5	29,64	0,040	S	1	Fair.
	2	0	67	64	29,64		SW by S	2	Cloudy.
18	7	0	55	63	29,71		WSW	1	Cloudy.
	2	0	69	64	29,70		SW	1	Fine.
19	7	0	57	63	29,71		SSW	1	Fine.
	2	0	71	64,5	29,71		S	1	Fair.
20	7	0	60	64	29,85				Hazy.
	2	0	69	65	29,87				Cloudy.
21	7	0	60	64	29,96				Fine.
	2	0	70	65	29,94		SSW	1	Cloudy.
22	7	0	58	64	29,74	0,376	SSW	2	Fair.
	2	0	62	64,5	29,70		SSW	2	Cloudy.
23	7	0	56	63,5	29,63	0,060	WSW	1	Cloudy.
	2	0	62	63,5	29,68		WSW	1	Cloudy.
24	7	0	53	63	29,87	0,058	WSW	1	Fine.
	2	0	63	63	29,86		SW	1	Cloudy.
25	7	0	56	63	29,83		SSW	1	Cloudy.
	2	0	62	63	29,78		SSW	1	Cloudy.
26	7	0	58	63,5	29,75		W	1	Cloudy.
	2	0	67	64,5	29,78		WSW	1	Fine.
27	7	0	57	63,5	29,78		WSW	1	Hazy.
	2	0	69	65	29,78		W	1	Hazy.
28	7	0	57	63,5	29,86	0,050	WNW	1	Cloudy.
	2	0	67	64,5	29,99		NW	2	Cloudy.
29	7	0	55	63,5	30,07		W	1	Fine.
	2	0	71	65	30,00		W	1	Hazy.
30	7	0	58	64	30,08	0,048	W	1	Fair.
	2	0	70	65	30,08		WNW	1	Fair.
31	7	0	57,5	63,5	30,01		WSW	1	Cloudy.
	2	0	68	64,5	29,85		S	1	Rain.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	S.r.	Weather.
Aug. 1	7	0	56	63	29,80	0,250	WSW	2	Cloudy.
	2	0	67,5	65	29,95		W	1	Cloudy.
2	7	0	55	63	30,11		N	1	Cloudy.
	2	0	64	63,5	30,11		N	1	Fair.
3	7	0	59	63,5	30,11		N	1	Cloudy.
	2	0	70	65,5	30,08		N	1	Fair.
4	7	0	60	64,5	30,05		E	1	Fine.
	2	0	72	67	30,00		E	1	Fine.
5	7	0	66	67	29,95		SW	1	Fair.
	2	0	72,5	67	30,08		SW	2	Cloudy.
6	7	0	61	67	30,08		SW	2	Fair.
	2	0	70	67,5	30,18		WSW	1	Fine.
7	7	0	57	66	30,27		W	1	Fine.
	2	0	70	67,5	30,27		W	1	Fine.
8	7	0	57	66	30,23		NNW	1	Fine.
	2	0	72	67,5	30,20		E	1	Fine.
9	7	0	59	66,5	30,19		E	1	Fine.
	2	0	70	67	30,19		E	2	Fine.
10	7	0	60	66,5	30,18		E	1	Hazy.
	2	0	72	69	30,17		E	1	Fine.
11	7	0	58	67	30,17		E	1	Fine.
	2	0	70,5	68,5	30,17		E	1	Fine.
12	7	0	58	67	30,15		ENE	1	Cloudy.
	2	0	71	69	30,14		E	1	Fine.
13	7	0	59,5	66,5	30,12	0,170	E	1	Fine.
	2	0	72,5	69	30,12		E	1	Fine.
14	7	0	59,5	66	30,12		NE	1	Cloudy.
	2	0	70	69	30,10		E	1	Fine.
15	7	0	59	66	30,09				Cloudy.
	2	0	72	68,5	30,06		NNW	1	Fine.
16	7	0	62	66	30,13		N	1	Fine.
	2	0	66	66,5	30,18		N	2	Rain.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 17	7	0	58,5	64,5	30,25	0,147	N	2	Fair.
	2	0	70	68	30,27		N	2	Cloudy.
18	7	0	60	64,5	30,33		NNW	1	Fair.
	2	0	68,5	67	30,30		ENE	1	Fine.
19	7	0	59	64,5	30,26		NE	1	Cloudy.
	2	0	69	68	30,17		E	1	Fair.
20	7	0	63	66	29,97		E	1	Fair.
	2	0	73	69,5	29,86		E	1	Fair.
21	7	0	63	66,5	29,73		W	1	Cloudy.
	2	0	68	68,5	29,70		W	1	Fair.
22	7	0	56	64,5	29,70	0,343	W	1	Fair.
	2	0	62,5	67	29,76		WSW	1	Fine.
23	7	0	53	62	29,92	0,438	W	1	Fine.
	2	0	63	65	29,98		W	1	Fair.
24	7	0	59	63,5	30,14		SW	1	Fine.
	2	0	66	65	30,16		SW	1	Cloudy.
25	7	0	60	65	30,18		WSW	1	Fair.
	2	0	66	65,5	30,15		SW	1	Fair.
26	7	0	58	64	30,10		SW	1	Fair.
	2	0	66	64,5	30,11		SW	1	Cloudy.
27	7	0	52	64	30,10		SW	1	Foggy.
	2	0	63	66	30,10		SW	1	Fair.
28	7	0	54	64,5	30,03		ESE	1	Fine.
	2	0	66	66,5	29,95		ESE	1	Cloudy.
29	7	0	60	65	29,84		E	1	Fine.
	2	0	74,5	69	29,72		SSE	2	Fine.
30	7	0	60	66	29,73				Cloudy.
	2	0	67	67,5	29,72				Cloudy.
31	7	0	58	66,5	29,70	0,516			Cloudy.
	2	0	68	68	29,70		SSE	1	Cloudy.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Sept. 1	7	o	57	66	29,69	0,465	SE	1	Rain.
	2	o	66,5	66,5	29,66		ESE	2	Cloudy.
2	7	o	57	66	29,57	0,295	SSE	1	Fair.
	2	o	66	67	29,58		S by W	2	Fair.
3	7	o	58	66	29,57		ESE	2	Fair.
	2	o	70	67	29,48		E	2	Cloudy.
4	7	o	58	65,5	29,37	0,197	SE	2	Rain.
	2	o	58	65,5	29,42		S	2	Cloudy.
5	7	o	57	65	29,73	0,140	SW	2	Fair.
	2	o	64	67	29,80		SW	1	Fair.
6	7	o	58	65	29,92		WSW	1	Cloudy.
	2	o	65,5	66,5	29,96		WSW	1	Fair.
7	7	o	58,5	65	30,03		WSW	1	Cloudy.
	2	o	65	67	30,03		WSW	1	Fair.
8	7	o	58	65	30,09		WSW	1	Cloudy.
	2	o	65	66,5	30,11		WSW	1	Fair.
9	7	o	58	64,5	30,15		WSW	2	Fair.
	2	o	67	66,5	30,15		WSW	2	Fine.
10	7	o	59	65	30,00		WSW	1	Cloudy.
	2	o	74	68	29,88		WSW	1	Cloudy.
11	7	o	59	67	29,90	0,150	W by N	1	Rain.
	2	o	61	67	30,06		W by N	2	Fine.
12	7	o	51	64,5	30,36	0,020	WNW	1	Fine.
	2	o	64,5	66	30,38		WNW	1	Fair.
13	7	o	56	65	30,25		SW	1	Fine.
	2	o	64	65,5	30,17		SW	1	Fair.
14	7	o	56	64,5	29,92		WSW	1	Cloudy.
	2	o	61	65	29,80		WSW	1	Cloudy.
15	7	o	48	62,5	29,82	0,200	W by N	1	Fine.
	2	o	57	62	29,76		WSW	1	Cloudy.
16	7	o	48	59	29,74		WNW	2	Cloudy.
	2	o	55	61,5	29,78		WNW	1	Fair.

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for September 1789.

1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Sept. 17	7	0	45	59	29,90	0,105	NWbyW	1	Cloudy.
	2	0	54	60,5	29,92		NW	2	Fine.
18	7	0	45	58,5	29,77		W	1	Cloudy.
	2	0	58	59	29,59		W	1	Cloudy.
19	7	0	47	58	29,38		W	1	Fine.
	2	0	56	60	29,30		WNW	1	Fine.
20	7	0	51	59	29,34	0,155	N	1	Rain.
	2	0	57	60	29,43		NW	1	Cloudy.
21	7	0	50	59	29,80		N	1	Cloudy.
	2	0	58,5	60	29,89		N	1	Fine.
22	7	0	50	58,5	30,08		NNE	1	Cloudy.
	2	0	60	60	30,05		N	1	Cloudy.
23	7	0	54	58,5	30,00		N	1	Fair.
	2	0	60	59,5	30,05		NW	1	Cloudy.
24	7	0	48	58,5	30,22		NWbyW	1	Fair.
	2	0	58	59,5	30,25		WNW	1	Fine.
25	7	0	49	58,5	30,24		WSW	1	Fine.
	2	0	61	60,5	30,24		WSW	1	Fine.
26	7	0	52	59,5	30,22		W	1	Cloudy.
	2	0	63	61,5	30,22		WSW	1	Cloudy.
27	7	0	50	59,5	30,08		WSW	1	Fine.
	2	0	64	61,5	30,00		WSW	1	Fine.
28	7	0	50	59,5	30,00	0,075	WSW	1	Fine.
	2	0	60	61	29,95		WSW	1	Cloudy.
29	7	0	58	60	29,69		SW	2	Cloudy.
	2	0	61	61,5	29,63		SW	2	Cloudy.
30	7	0	46	59	29,68	0,353	WSW	2	Fine.
	2	0	54,5	59,5	29,72		WSW	2	Cloudy.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Oct. 1	7	o	50	59	29,20	0,485	SW	2	Cloudy.
	2	o	52	59	29,19		WSW	2	Cloudy.
2	7	o	46	57,5	29,26	0,133	W	2	Fine.
	2	o	51,5	59	29,16		W	2	Fair.
3	7	o	46	57	29,05	0,063	W	2	Fair.
	2	o	55	58	29,15		W	2	Fair.
4	7	o	43	55	29,45		WNW	1	Cloudy.
	2	o	50	56	29,53		WNW	1	Fair.
5	7	o	46	53	29,35	0,020	SW	1	Cloudy.
	2	o	48,5	53,5	29,14		SSW	2	Cloudy.
6	7	o	42	53	29,00	0,275	W	2	Cloudy.
	2	o	54	58	29,05		WSW	2	Fair.
7	7	o	45	55	29,27	0,053	W	2	Fine.
	2	o	53	58	29,32		W	2	Fair.
8	7	o	48	55	29,20	0,204	S	1	Rain.
	2	o	50	57	29,14		S	1	Rain.
9	7	o	45	56	29,29	0,762			Cloudy.
	2	o	51,5	58,5	29,35		SW	1	Fair.
10	7	o	45,5	56,5	29,48		SSE	1	Fine.
	2	o	53	59	29,52		S by W	1	Fair.
11	7	o	45	57	29,83		S by W	1	Fine.
	2	o	53,5	58	29,79		S by W	1	Fair.
12	7	o	49,5	56,5	29,48	0,635	W	1	Rain.
	2	o	54	58	29,55		SW	1	Fair.
13	7	o	46	57	29,52		SSE	1	Cloudy.
	2	o	55	58,5	29,40		SSE	2	Cloudy.
14	7	o	40,5	56,5	29,38		SE	1	Foggy.
	2	o	52,5	58,5	29,42		ESE	1	Cloudy.
15	7	o	47	57	29,33	0,391	ESE	1	Cloudy.
	2	o	52	57,5	29,42		WNW	1	Cloudy.
16	7	o	42	56	29,67		WNW	1	Fine.
	2	o	53	59	29,77		WNW	1	Fine.

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1789	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Oct. 17	7	0	43	56	29,92		WNW	1	Fair.
	2	0	52,5	59	29,90		SW	1	Cloudy.
18	7	0	50	57	29,66		SSE	1	Cloudy.
	2	0	55	59	29,68		SSW	1	Cloudy.
19	7	0	43	56,5	29,78	0,026	S	1	Foggy.
	2	0	55	58,5	29,74		SSW	2	Cloudy.
20	7	0	57	58	29,79	0,072	SW	2	Cloudy.
	2	0	59	62	29,87		SW	2	Fine.
21	7	0	52	58,5	29,90		SSW	1	Fine.
	2	0	59	63	29,83		SW	1	Fine.
22	7	0	51	58	30,01		WNW	1	Cloudy.
	2	0	53	60	30,04		N	1	Cloudy.
23	7	0	51	58	30,07		NE	1	Cloudy.
	2	0	57,5	61,5	30,08		ENE	1	Cloudy.
24	7	0	50	60	30,20		ENE	1	Cloudy.
	2	0	53	61,5	30,24		ENE	1	Cloudy.
25	7	0	47	58,5	30,26		NE	1	Cloudy.
	2	0	51	59	30,24		ENE	1	Cloudy.
26	7	0	47	57	30,24		N	1	Cloudy.
	2	0	52	59	30,22		NE	1	Cloudy.
27	7	0	44	57	30,25		NW	1	Cloudy.
	2	0	46	58	30,29		NNW	1	Cloudy.
28	7	0	43,5	57,5	30,25		SW	1	Foggy.
	2	0	50	58	30,22		W	1	Cloudy.
29	7	0	44	57	30,12		WNW	1	Cloudy.
	2	0	49	58	30,10		W	1	Fair.
30	7	0	40	55	30,03		W	1	Fair.
	2	0	45	56	29,87		W	1	Cloudy.
31	7	0	36	53,5	29,87	0,140	NNE	2	Fair.
	2	0	39	54,5	30,11		NNE	2	Fine.

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1789	Time. H.	M.	Therm. without °	Therm. within °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 1	7	0	34	52	30,30		NW	1	Fine.
	2	0	41	54	30,30		NW	1	Hazy.
2	7	0	44	52	29,60		SW	2	Cloudy.
	2	0	48	53,5	29,43		SW	2	Cloudy.
3	7	0	35,5	52	29,18	0,010			Foggy.
	2	0	46	54	29,08		SW by S	1	Cloudy.
4	7	0	44,5	52,5	29,00	0,100	S by W	1	Cloudy.
	2	0	47	54,5	29,10		S by W	1	Fair.
5	7	0	44	52,5	29,25	0,082	SSE	1	Cloudy.
	2	0	47	54	29,25		SSE	1	Cloudy.
6	7	0	36	52,5	28,81	0,180	N	1	Cloudy.
	2	0	41	53	28,74		N	1	Cloudy.
7	7	0	37	52	28,72		N	1	Cloudy.
	2	0	41	52	28,77		NW	1	Cloudy.
8	7	0	39	51	29,07	0,112	NNW	1	Cloudy.
	2	0	45	53	29,15		NW	1	Cloudy.
9	7	0	37	51,5	29,47		W	1	Fine.
	2	0	44,5	54	29,57		W	1	Fair.
10	7	0	31,5	50	29,76		W	1	Fine.
	2	0	40	53	29,81		W	1	Fine.
11	7	0	34	50,5	29,93		W	1	Fine.
	2	0	44	53	29,93		W	1	Fine.
12	7	0	42	52	29,65	0,110	SW by W	1	Cloudy.
	2	0	44	53,5	29,64		WSW	1	Cloudy.
13	7	0	38	52	29,72		WSW	1	Cloudy.
	2	0	49	53,5	29,56		SW	1	Cloudy.
14	7	0	42	52,5	29,65	0,030	SW	1	Cloudy.
	2	0	55	56	29,41		SW	2	Cloudy.
15	7	0	45	53	29,42	0,078	SW	1	Rain.
	2	0	51	55	29,30		SW	2	Cloudy.
16	7	0	41	53	29,44	0,190	SW	1	Fair.
	2	0	48	56	29,46		SW	1	Fair.

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1789	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 17	7	0	43	53,5	29,45	0,020	SSW	1	Fair.
	2	0	46	55,5	29,43		SSW	1	Cloudy.
18	7	0	37	53	29,51	0,126	E	1	Foggy.
	2	0	44,5	54,5	29,53		SW	1	Fair.
19	7	0	40	53	29,68		ENE	1	Cloudy.
	2	0	45	54	29,71		NE	1	Cloudy.
20	7	0	36	53	29,64		W	1	Fair.
	2	0	46	54	29,71		W	1	Cloudy.
21	7	0	37	52	29,90		W	1	Cloudy.
	2	0	44	54	29,82		W	1	Cloudy.
22	7	0	42	52	29,87	0,100	NNE	1	Cloudy.
	2	0	46	55	29,92		N	1	Cloudy.
23	7	0	38	52,5	30,11	0,070	NNE	1	Cloudy.
	2	0	44	53,5	30,15		NNE	1	Cloudy.
24	7	0	36	52	30,34		N	1	Cloudy.
	2	0	40	53	30,27		WNW	1	Cloudy.
25	7	0	36	51	30,20		W	1	Cloudy.
	2	0	41	53	30,22		W	1	Cloudy.
26	7	0	36	51,5	30,28	0,036	E	1	Cloudy.
	2	0	37	53	30,35		E	1	Fair.
27	7	0	28,5	50	30,46		W	1	Cloudy.
	2	0	40	51	30,42		W	1	Fair.
28	7	0	33,5	50	30,42		W	1	Foggy.
	2	0	36	50	30,42		W	1	Cloudy.
29	7	0	35	48	30,37		W	1	Cloudy.
	2	0	40	50	30,32		E	1	Cloudy.
30	7	0	37,5	48,5	30,08		S	1	Cloudy.
	2	0	38	50	29,96		E	1	Fair.

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1789	Time. H.	M.	Therm. without	Therm. within.	Barom.	Rain.	Winds. Points.	Str.	Weather.
			°	°	Inches.	Inch.			
Dec. 1	8	0	36	48	29,70		E	1	Fair.
	2	0	44	51	29,66		S	1	Fair.
2	8	0	44	49,5	29,60		S	1	Cloudy.
	2	0	46	53,5	29,68		SW	1	Fair.
3	8	0	35	50	29,97		WSW	1	Foggy.
	2	0	37	51,5	30,02		W	1	Foggy.
4	8	0	41	51	30,15		WSW	1	Cloudy.
	2	0	47	54	30,16		SW	1	Cloudy.
5	8	0	47,5	54	30,27		SW	1	Cloudy.
	2	0	51	55	30,26		WSW	1	Cloudy.
6	8	0	47	54	30,38		WSW	1	Cloudy.
	2	0	50,5	58	30,41		WSW	1	Cloudy.
7	8	0	40	54	30,47		W	1	Fine.
	2	0	45,5	55,5	30,50		W	1	Cloudy.
8	8	0	37	52	30,50		W	1	Cloudy.
	2	0	40	53	30,50		W	1	Cloudy.
9	8	0	33	51,5	30,56		W	1	Cloudy.
	2	0	33,5	53	30,56		W	1	Cloudy.
10	8	0	37	51	30,54		W	1	Cloudy.
	2	0	42,5	53	30,54		W	1	Cloudy.
11	8	0	41	52	30,53		W	1	Cloudy.
	2	0	43	53	30,51		W	1	Cloudy.
12	8	0	43	52	30,38		W	1	Cloudy.
	2	0	46	54,5	30,38		W	1	Cloudy.
13	8	0	43	53	30,33		WSW	1	Cloudy.
	2	0	47	54,5	30,21		SSW	1	Cloudy.
14	8	0	37,5	52	29,82		E	1	Cloudy.
	2	0	42	54	29,71		E	1	Cloudy.
15	8	0	45	52,5	28,88	0,110	W	2	Cloudy.
	2	0	45,5	54	28,97		W	2	Cloudy.
16	8	0	34,5	50,5	29,18	0,045	WNW	2	Fine.
	2	0	38	53,5	29,14		WNW	2	Cloudy.

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1789	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Dec. 17	8	0	36	51	29,10		WNW	2	Cloudy.
	2	0	43,5	53	29,31		WNW	2	Cloudy.
18	8	0	36	52	30,02		WNW	1	Fine.
	2	0	42	53,5	30,03		WSW	1	Fine.
19	8	0	43	53	29,70		WSW	2	Cloudy.
	2	0	47	55	29,54		SW	2	Cloudy.
20	8	0	40	51,5	29,70	0,165	WNW	2	Cloudy.
	2	0	47	54,5	29,74		WNW	1	Cloudy.
21	8	0	44	53	29,80		WSW	1	Cloudy.
	2	0	47	55	29,74		SW	1	Cloudy.
22	8	0	50,5	54	29,54	0,053	SW	2	Cloudy.
	2	0	53	57	29,47		SW	2	Cloudy.
23	8	0	48	55	29,78	0,055	SW	2	Cloudy.
	2	0	51	57	29,72		SSW	2	Cloudy.
24	8	0	50	56	29,24	0,236	S	2	Rain.
	2	0	44	57,5	29,21		SSW	2	Rain.
25	8	0	42	55	29,40	0,337	SW	2	Rain.
	2	0	44	56	29,44		W	2	Cloudy.
26	8	0	33	53	29,78		WNW	1	Fine.
	2	0	39	56	29,76		WNW	1	Fine.
27	8	0	47	53	29,88		W	2	Cloudy.
	2	0	47	54,5	29,82		SW	2	Rain.
28	8	0	45	54	29,87	0,100	W	2	Fine.
	2	0	47	55	29,98		WNW	2	Fine.
29	8	0	49	54	29,75	0,036	SW	2	Rain.
	2	0	52	57	29,66		SSW	2	Cloudy.
30	8	0	45	54	29,76		SW	1	Cloudy.
	2	0	50,5	55,5	29,63		SSW	2	Cloudy.
31	8	0	46	55	29,30	0,053	SSE	2	Cloudy.
	2	0	47	56,5	29,21		S by E	2	Cloudy.

	Thermometer without.			Thermometer within.			Barometer.			Rain.
1789	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inch e
January	53,0	17,5	35.7	56	36	46	30,75	28,58	29,72	1,345
February	51,0	34	42,5	57	49	52,5	30,34	28,65	29,70	1,605
March	46,5	26	36,6	52	44	478	30,13	28,94	29,72	1,549
April	62	29	47,2	61	48	54,2	30,18	29,10	29,77	0,957
May	66	45	56,9	65,5	55	60,5	30,27	29,57	29,88	1,103
June	72	50	58,5	66	58,5	61,5	30,23	29,40	29,84	3,244
July	71	52	61,9	65	59	63.5	30,09	29,54	29,85	2,467
Auguſt	74,5	54	63,7	69	62	66,2	30,33	29,70	30,06	1,864
September	74	45	57,3	68	58,5	62,6	30,38	29,30	29,88	2,155
October	59	36	49,1	63	53	57,5	30,29	29,00	29,52	3.253
November	55	28,5	41,0	56	48	52,6	30,46	28,72	29.70	1,244
December	53	33	43.5	58	48	53.6	30,56	28,88	29,86	1,190
Whole year			49.5			56.5			29,79	21,976

END OF PART I. OF VOL. LXXX.